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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—OTHER MINDS (I.).

BY JOHN WISDOM.

1. *Natural doubts and philosophical doubts.* At a recent meeting of the Moral Sciences Club at Cambridge, Mr. Isaiah Berlin read a paper in which he raised, in a very interesting way, what I venture to call the Inner-Outer metaphysical doubts. At this meeting we contrasted two cases of doubt as to whether a certain man, Smith say, believes that flowers feel, namely (1) a case of *natural* doubt arising because the doubter knows only that Smith has once at a party said, "I believe flowers feel," and thinks that Smith might have been saying this for the sake of something to say; and (2) a case of *philosophical* doubt arising because the doubter knows only *outward* signs of Smith's *inward* state, and feels that from these he can never be sure what that state is.

2. *Two sources of philosophical doubt.* This philosophical doubt as to Smith's inward state must be distinguished from another philosophical doubt as to his inner state,[1] one which

[1] Or, if you like, the doubt is the same and we have to distinguish between two sources of the philosophical doubt. It was Wittgenstein who first drew my attention to the need to distinguish between these two sources of doubt about other people's states of mind.

How much in this paper is due to Wittgenstein will be appreciated only by people who have listened to him. My debt to him is enormous and is by no means to be measured by the few places where I happen to mention that such and such a point comes from him or put a W. against an example

arises from the infinite corrigibility of " Smith believes that flowers feel ", an infinite corrigibility which it shares with any

of his. At the same time I do not think that my way of doing things would quite meet with his approval—it's not sufficiently hard working—a bit cheap and flash.

I make no apology for mentioning this sort of point. For this is the sort of criticism of philosophical work which I find appropriate. Those who deplore so ' personal ' an attitude and say, " Who cares whether so and so likes what is said, what we want to know is whether it is true ", emphasise the objectivity of philosophy to the point of turning it into a science. I remember with what relish I once heard MacTaggart say in a discussion, " What we want to know is not why he said it but what reason he had for saying it ". But I didn't then realise how near to reasons are some causes which aren't reasons and how beside the point are many reasons.

To continue, I want to say : There are three sources of chronic doubt as to whether S is P. 1. The doubt may arise from the infinity of the criteria as to whether S is P. Because of the infinity of the criteria it can never be said that all are present. At the same time there are no groups, short of all, of which we unhesitatingly say, " With these enough are present ". This is the philosophical doubt most directly connected with an anxiety doubt, " Are the taps turned off ? ", " Have I turned out the lights ? ", and thus with obsessional duties, " I must go and make sure I have turned out the lights, turned off the taps, locked the doors, got a visa for heaven, made sure of security ". Is this water or a mirage ? Is this love or infatuation ? I don't yet know. I can't ever know. I don't know with Annabel, I can never know with anyone, How I'll feel to-morrow, How she'll feel to-morrow, Whether it's really love.

2. The doubt may arise from conflict among the criteria for S's being P. " Can you play chess without the queen ? " (W.). " Is a tomato a fruit or a vegetable ? " (W.). " Can you unintentionally keep a promise ? "

3. The doubt may arise from hesitation at the jump from the criteria, even the infinite all, to S is P, *e.g.* from the outer (bodily) to the inner (spiritual), from the present to the past, from the actual to the potential, from possibilities of sensation to physical things. These are the doubts which may take form in the idea of an arch-deceiver whose performance always goes without a hitch.

There is a connexion between (1) and (2) which is indicated by the phrase, " There is no definite line where we say that enough criteria are present for us to say that S is P ". There is a connexion between (2) and (3) in that hesitation as to whether to doubt in the way the third class of sceptic doubts is hesitation as to whether, in view of conflicting criteria, one should say with these sceptics that s_1, s_2 . . . s_∞ entail or mean that S is P or not—conflicting criteria, I mean, for the presence of entailment or identity of meaning.

A doubt such as " I can never really know what another person is feeling " may arise from more than one of these sources. This over-determination of sceptical symptoms complicates their cure. The treatment is like psycho-analytic treatment (to enlarge on Wittgenstein's analogy) in that the treatment is the diagnosis and the diagnosis is the description, the very full description, of the symptoms.

What I mean is this: When in psycho-analysis it is said of a patient " He is like this because when he was a child he wished for sexual contact

statement about people and things, indeed any statement having infinite corrigibility! The difference between these two philosophical doubts is sometimes brought out thus: The inner-outer doubter is saying that even though he knows not only what Smith's behaviour has actually been but also what it will be and what it would be if this or that were to happen and what it would have been if this or that had happened, that even though he has all this knowledge, and it is all in favour of Smith's believing that flowers feel, still he doesn't know that Smith believes that flowers feel. With such extensive knowledge he is provided against surprise but not against the fact that his evidence is about outward and visible things while his conclusion is about an inward and invisible state.

3. *Another chronic doubt.* Both the philosophical doubts just described must be distinguished from and likened to another sort

with his mother", this means, "If we analyse him, *i.e.* let him talk on, he will talk in such and such a way and then feel better".

It may be said that the analyst cures by recommending a certain view of the patient's case. But the explaining to the patient what is wrong with him consists in allowing the patient to describe his own case, but not in "septic" general terms, such as "I have always suffered from a weak heart", "I have always been treated unfairly", but in an "aseptic" story language, *e.g.*, "I remember once when I was carrying all the things for a picnic and my sister wasn't carrying anything, suddenly at the top of the hill I felt a stabbing pain over my heart. . . ."

In like way a philosophical difficulty such as "What then, do you say that philosophical questions are meaningless?", "Do we know that dogs hear whistles too high for us to hear?" may be removed by getting the patient to describe himself. He must then describe fully the sort of question or statement he is considering. By the time he has described this, *e.g.*, has described a philosophical question, he has set it in its place on the language map with regard to all other questions. And thus he has answered his own question. He is like the man who first saw a zebra and came back and said, "I saw an animal with hoofs like a donkey but in shape rather more like a pony than a donkey. What sort of animal was it? Would you say it was a kind of donkey?" His friend said, "Tell me, what sort of coat had it? Like a pony or like a donkey? Like a pony you say. Well I should say it was a sort of pony. But what sort of ears had it? Long ears? Oh! well then . . . But neighs like a horse?". And so on. It appears that the man who asked the question was best placed for answering it.

In the same way every philosophical question, when it isn't half asked, answers itself; when it is fully asked, answers itself.

Of course it is not necessary always to adopt this *entirely* "aseptic" technique, to borrow a metaphor from Dr. George Pegge, an analyst. (I have hinted at the meaning of "aseptic" above.) One may gently, surreptitiously, introduce a septic but beneficent notation which shall help towards the grasp and arrangement which so slowly comes from repeating again and again in this order and that the stories which give the setting of the patient's difficulties.

of doubt which I venture to call " philosophical," [1] though I cannot here present all the excuses for stretching the word to cover all doubts of this sort. Examples of doubts of this sort are, " Can a man keep a promise by mistake ? ", " Is a zebra without stripes a zebra ? "

A doubt of this sort may arise, of course, as to the presence of doubt or belief. Just as one may hesitate as to whether Annabel loves or hates Bertram because in some ways she does and in some ways she doesn't, so one may hesitate when asked whether Colonel Fawcett's wife really believes he's still alive somewhere in Brazil. True, she always swears she does when you ask, and no doubt she then honestly feels she does ; but it's so many years ago now that he disappeared, as she well knows ; and wouldn't she be as astonished as anyone to see him come up the garden path ? One may say, " She does and she doesn't believe it ".

[1] The excuse for doing so is that all philosophical questions register conflict, that all philosophical doubts are requests for decision. In note 1, page 369, on the sorts of chronic doubt, I have represented philosophical doubts as some of them conflict doubts and some not. Here I want to say that all are but some in one way and some in another. I must explain myself. The doubts I call *conflict doubts* in the note to page 369 are of the form, " Given s_1, s_2 . . . s_{n-m} and given that not s_{n-m} to s_n shall we say that Σ ? " I call this a request for a decision as to the use of ' Σ '. Here s_1 . . . s_n may be a typical group of criteria for Σ (a text-book case of schizo-phrenia) or may be a group of all consistent criteria for Σ. Examples in addition to those I have given are : " Is Jack's promise keeping promise keeping ? " (see p. 384) ; " Is philosophical doubt doubt ? " (see the whole of this paper).

The doubts I call on page 370 *doubts from infinite corrigibility* are of the form, " We know that s_1 . . . s_n but do not know that s_n . . . s_∞, do we know that Σ ? " where s_1 . . . s_n make a text-book case of Σ although s_n . . . s_∞ are other tests for Σ which have not been carried out. These are requests for decision as to the use of ' know ' *although ordinarily it would be unhesitatingly applied.*

The doubts I call on page 370 *doubts arising from the jump from criteria to that of which they are criteria* I now call requests for decision as to the use of ' means the same '.

But now a warning is necessary. From the fact that a man describes a non-typical and border line case of Σ and asks, " Is this Σ ? " one may well and wisely guess that he is requesting a decision on the use of ' Σ '. But this *may* not be so. On enquiry you may find that he regards himself as having described a typical case of Σ, so that what he is really concerned with is the relation between an infinite or utterly first class group of criteria for Σ and Σ. Thus, if a man describes a non-typical case of belief or love in behaviouristic terms and asks, " Is it belief ? ", " Is it love ? ", then you can't be sure whether what bothers him is (1) its non-typicalness, or (2) the jump from the outer criteria to the inner state, or (3) the everlasting possibility of something turning up which would make us waver as to whether there was real conviction, real love.

If someone asks of Annabel and Bertram "Does she love him or hate him?" someone, *even someone who knows them well*, may answer "I can't make out which it is". This difficulty, this ignorance, can be removed, not by saying "You *do* know", but by saying "Yes, you don't know. But what sort of not knowing is this?[1] It's like not knowing whether a leopard without spots is a leopard, whether an axe of sugar which breaks when you use it is an axe".

4. *The inner-outer doubts.* What concerned Mr. Berlin were some of the inner-outer doubts. "Surely from information, however complete, about a man's bodily condition and behaviour I can never know how he's feeling or even that he's feeling?" It is worth noticing how these doubts as to the quality, and finally as to the existence, of "experiences" behind the faces of others may grow from a milder scepticism which amounts to no more than a relativity in psychology. One may say: "We can never tell the absolute quantities of desire and feeling which others have. We can't, for example, be sure that Napoleon got a very great thrill from success. True, we know that he was prepared to sacrifice thousands of lives in order to gain it. But then all this proves is that the ratio of his desire for success to his desire for the safety of his fellows was abnormal, and this may have been because he didn't care as much about his fellows as most people do, and not because he had a very strong desire for success. True, he showed signs of concern for his fellows when riding over the battlefield, but then perhaps he was a man who *showed* such feelings more readily than most men in those days. True, he divorced Josephine. But then this only proves that the ratio of his desire for success to his love for one he loved best wasn't normal. And this may have been because he didn't love anyone *very* much, and not because he loved success more than you or I. Maybe you and I want success just as much, only from love, or nobly, we resist our inclinations to set this before the welfare of those near to us. No, we can't tell, not for certain at any rate, whether it is that Smith feels things more intensely than Jones or that he gives vent to them more readily. We can't say: 'He loves music very much'; only that 'the ratio of his desire for music to his other desires and aversions is abnormally high'."

Having said all this, then very readily the embarrassing idea

[1] The line between a man who asks, "Does she love him?" as an empirical question and one who asks it as an *a priori* question is not sharp. For a man may be induced to decide partly by giving him new information and partly by reminding him of information he has already.

occurs to one that for all we know the very feeling which we call aversion and which makes us act in one way is what makes Smith act as if he desired a thing. Specialists once were puzzled by a girl in Vienna who cried when she was glad and laughed when she was sad. How did they know she was sad ? They watched her eyes and so on. But might not her eyes and all her other reactions have been misleading like her laughter ? Unlikely, of course, and of course it would then never be found out. But just as there is nothing impossible in *some* of the reactions usually associated with happiness being associated with unhappiness, so there is nothing impossible in *all* of the reactions associated with happiness being associated with unhappiness. And if this happened in the case of a man called Smith, say, why then we should never know. We should clap him on the back and congratulate him just when, though he was grinning and kept trying for more of what he'd had, he was feeling wretched. Something like this does sometimes nearly happen, and then hardly anyone knows. And surely a man might just be able to tell *no* one that really it made him wretched. And if he didn't *no* one would know. So we don't know but what many of our friends are like this all unknown to us. It may be unlikely but we can't be certain [1] it isn't so. How can we tell, for example, whether the colour which makes Smith call a thing red is the colour which makes us call a thing red ? If he saw green just where we saw red and vice versa we should never know. He would match things just as we do, and if asked to paint something would paint it so it looked green to him, that is so it would look red to us. So we shouldn't know there was anything wrong. When Smith says he sees a red flag all we know is that he sees a flag of a colour which stops him and which he calls red. But we don't know what colour he sees. And when Smith says he believes that flowers feel, then, however consistently he acts, how can we be sure that he isn't pulling our leg and never slipping up on it ? We can't be sure. There must always be some doubt, practically negligible if you like, but still some doubt as to what is going on in the mind of another. What lies behind the smiling face, pleasure, or pain, or nothing ?

5. *But now what sort of ignorance is this ? What sort of doubt ?* If we try to use the doubt it turns to dust. If we say, " So perhaps Smith won't sigh when he hears the young plants are dying ", then the Sceptic says, " No, I am sure he'll sigh ". The usual case in which we speak of doubting is one in which the doubt does not turn to dust when it is used. But the sceptic

[1] And if it isn't ever certain how can it sometimes be probable that behind A is α ?

doesn't even half expect to find Smith treating flowers callously nor to find in his diary after he is dead the words, " Made them all think I thought flowers feel ". He has explained that he expects nothing of the sort. But now isn't faith without works dead ? And isn't doubt without works dead ? " He swears that he loves her but he does nothing for her though she's so ill—its a queer sort of love." [1]

6. *The contemplating of a possibility ?* The Sceptic may say, " Well, perhaps my doubt is a queer doubt. It is not so much a doubt as the contemplating of a possibility."

7. *I must protest that this is a very inadequate description of this dead doubt.* It at once goes too far and not far enough. It represents the doubt as less like a live doubt than it is and also as more like. It represents the doubts, " Is there within this body a soul which controls it ? " and " Is there within this machine a man who manipulates it ? " as less like each other than they are and also as more like. It represents them as less like than they are because when someone says, " But can you be sure that there are any feelings behind the behaviour of others ? " I feel a qualm just like I feel when someone says, " You thought that was an attendant who nodded in answer to your question but it was an ingenious waxwork figure ".

On the other hand, to say that the philosophical doubt is the contemplating of a possibility makes it too like a live doubt. The doubt as to whether the figure was an attendant or a waxwork can be settled by watching, by further experiments, though maybe we shan't have time or opportunity to do them. But the doubt whether within Smith's body is a soul cannot be settled by further watching and listening. For, of course, watching and

[1] Another way of treating this would be to say : I doubt whether you doubt whether Smith believes flowers feel. Consider my doubt as to your doubt. Of what am I ignorant about you ? I doubt (don't you ?) whether my doubt is doubt. Now our doubt about my doubt is also not a matter of ignorance. Your doubt about Smith may be like ours because just as we are in doubt because we are inclined to say both that I doubt and that I don't, so you may be inclined to say both that Smith believes flowers feel and that he doesn't. This will be the case if your doubt arises because Smith's doubt is philosophical and peculiar and queer. If it doesn't arise like this but arises like the doubt as to whether he is in pain because he groans, etc., then it is less like our doubt, which arises because of the queerness of my doubt, and less like my doubt about you, which arises in the same way. But still your doubt will be like ours in that it is peculiar in that it is not a matter of ignorance, not a matter of not knowing what to expect of Smith.

It may be replied, " But my doubt as to Smith *is* a matter of ignorance ". But this point in these discussions will be reached later in the text.

listening would reveal only further outward signs. Yet unfortunately no other means are available. It isn't that there is another means, a psychometer or telepathiser say, which with other people tells us what their inner states are but isn't available for Smith because of the expense. On the contrary, there is no test which *if we did carry it out* would tell us Smith's inner state. Our ignorance is not removable by watching his face under a microscope nor by questioning him for hours nor by waiting in a dark room for a telepathic revelation.[1] This is why I say that to speak of our doubt as *the contemplating of a possibility, the imagining of a possibility*, makes it look more like than it is to the doubt whether this figure is an attendant or a waxwork. For this latter doubt *is* connected with the possibility that further experiments on the figure will give results against saying that he is an attendant, it *is* connected with the possibility that though he answers " This way to the chamber of horrors ? " with a nod, he fails to answer any other question. And just as I call the doubt whether Smith believes flowers feel, when he is known to have given infinite and perfect evidence of belief that flowers feel, a queer doubt, so I call the contemplating of the possibility that Smith does not believe flowers feel, a queer contemplating of a possibility—What possibility ?

The answer sometimes given is, " The possibility that Smith does not believe that flowers feel, that his inward state does not fit his outward behaviour ". And this same answer may be given to " What is in doubt ? "

But there is sometbing very dissatisfying about this answer. The Sceptic says, " I doubt whether he really believes that flowers feel ". We say, " You mean that possibly if we look in his diary after he's dead . . . ? " The Sceptic says, " No ". We say, " Then what is it you have in mind ? ". The Sceptic says, " I have in mind the possibility that after all he doesn't believe that flowers feel ". Suppose a Sceptic says, " I doubt whether these nations are at war ". We say, " You mean that possibly although guns are being placed in position and aeroplanes are flying overhead the gunners will all refuse to fire when ordered to do so ? " The Sceptic says, " No ". We say, " What then ? " The Sceptic says, " All we know is that these men are moving these guns about. But after all the nations are not these men. I am just doubting whether the nations themselves are at war ".

[1] Or it is removable in this way. And then it is. And that is the solution. Then to say, " We don't know what is in the minds of others " is to say, " We haven't the power of telepathy ". The answer to this empirical lament is, " Some of us have sometimes ", or " Yes, isn't it a pity ", or " Yes, isn't it a good thing ".

What does he mean ? What is he doubting ? He expects all we expect. Is he really doubting ?

Suppose someone says of Smith who, after a long illness, breathes his last, "I wonder if he's dead". We say, "You mean, is it a sort of coma, a sort of suspended animation ? ". He says, "No, I don't mean he will come to life again, as we say. I mean is the essential Smith perhaps alive all the time. I know in the ordinary way we should say he's dead. But after all, all we know is that his body no longer moves up and down in breathing and that it won't do so. But Smith is not just his body, and I am wondering whether Smith still exists." What does this Sceptic mean ? What does he expect which we don't expect ? Is he doubting ?

And here remember that this issue which I, who am not a sceptic, am raising with regard to the inner-outer sceptic, is not the issue which, as a sceptic, he is raising with regard to Smith. It happens from our selecting as an example of a statement about Smith's mind, "Smith believes, etc.", that the same or nearly the same form of words express our doubts. But they are very different doubts. The Sceptic says, "Smith gives all the normal signs of doubting the usually accepted view that flowers don't feel and I am sure that he always will give all the usual signs of doubting this, but does he doubt it ? ". I say, "Does the Sceptic, the philosophical sceptic, really doubt about the minds of others ? " But I *don't* say the philosophical sceptic gives all the normal signs of doubting.[1] On the contrary, what I say is, "He looks and feels *at the moment* as if he is doubting but the feeling will not in the usual way be accompanied by any hesitancy in preparations for the future or surprise or relief as it unrolls ". Jones while at the revival meeting sweats with conviction of eternal life but his belief isn't real, because in the morning he is afraid to die. Or shall we say that his conviction was real but very evanescent ? Shall we say, "It wasn't really love " ?, or shall we say, "It was love but love is so evanescent " ? (W.). Shall we say that McTaggart [2] doubted the existence of

[1] Wittgenstein was inclined to put my question in the form : "He gives none of the normal signs of doubt. Is he in doubt ? " But this confuses doubts (2) and (3) (see note 1 to page 369), although Wittgenstein himself drew my attention to the distinction. What Wittgenstein had in mind I think was that the Sceptic gives none of the *contextual* signs of doubt. He is still just as considerate of Smith's feelings although he says with an air of doubt that he doubts their existence.

[2] McTaggart, *Some Dogmas of Religion*; Moore, "Proof of the External World ", *Proceedings of the British Academy*, vol. xxv. To understand this we need to understand the James Lange theory of emotion and to extend it to believing, doubting, imagining.

matter but the doubt was very evanescent ?, or shall we say that he didn't really doubt the existence of matter ? When I said " They didn't have to amputate, I've still got both my hands " did he disbelieve this statement ?

8. *Let us put the issue with an example which is less confusing.* Suppose the Sceptic says : " Smith shows all signs of pain but is he really in pain ? I can't be sure. I can't get rid of this doubt. True, Smith gives such signs of pain as I am sure will never be followed by his saying ' I was pulling your leg '—there's an arrow in his chest, there is sweat on his brow. But even these are only outward signs. And surely I can *wonder whether* he feels the opposite of what I do under these circumstances or whether he doesn't feel at all. Surely I can *imagine* that when I see red, white and blue he sees black, green and yellow ? "

9. *Imagining the opposite inner state.* Who am I to say what the Sceptic can imagine ? No doubt he says he can imagine Smith sees the Union Jack as black, green and yellow and telephone boxes as black and grass as white, because he *does* imagine this. So do I, so can I. I imagine now that Prof. G. E. Moore sees things like this. How queer it would look to me. Yet, of course, it must seem quite ordinary to him and so it would to me if I had always seen things that way.

10. *Rival pictures and rival hypotheses.* But there is something queer about this imagining. Imagining that though Smith is moaning he is quite comfortable and pleased doesn't consist in having an image of Smith comfortable and pleased—for what I ordinarily call having an image of Smith comfortable and pleased includes an image of his smiling face. Nor does this imagining that Smith, in spite of *all* appearances, is comfortable, consist in having an image of Smith moaning and frowning and then when no one is looking grinning all over his face. No. It is the image (or faint sensation) of comfort which is the essential image in imagining Smith comfortable. Now this image is not related to what one is imagining in the way in which mostly one's image when one is imagining is related to what one is imagining. The image of comfort is related to one's imagining Smith being in comfort in very much the way that one's image of a leprechaun is related to what one is imagining when one imagines that in one's watch is an *invisible* leprechaun.

Smith, of course, when asked the colours of the Union Jack replies, " Red, white and blue " because he has been taught to call the colours he then sees ' red ', ' white ' and ' blue '. But does he see the same colours, red, white and blue, as we see or does he see, say, black, green and yellow ?

Here are rival pictures, one of the Union Jack in red, white and blue and one of it in black, green and yellow, underneath each is written 'Smith'. But these pictures are not related to my wondering whether Smith sees the Union Jack in red, white and blue or in black, green and yellow, in the way that my images of a rat and a fox are related to my wondering whether it's a rat or a fox that is causing the commotion in the hen-house. No, my pictures of the green, black and yellow Union Jack and the red, white and blue Union Jack, corresponding to the two hypotheses, (1) that its Smith's seeing the Union Jack in red white and blue, (2) that it is his seeing it in black, green and yellow which is responsible for his behaviour, help to work these hypotheses, more in the way that my pictures of a germ in his blood and of a poison in his veins work the hypotheses (1) that it is an *invisible* germ which is making him so ill, (2) that it is an *invisible* poison which is making him so ill. The pictures of the Union Jacks lead to no expectations of seeing Union Jacks inside Smith. Nor do the pictures of the germ and the poison.

11. *The leprechaun and brownie hypotheses with 'idle' pictures.* Suppose one person says, "This watch behaves so oddly because there's a leprechaun inside", and another says, "No, its a brownie". Of course these hypotheses are related to the leprechaun and brownie pictures in a way like the hypotheses about Smith's inner state are to the pictures of a green, black and yellow flag and red, white and blue flag, only if they lead to no expectations of seeing a leprechaun. That is, the leprechaun and brownie must be invisible like Smith's inner states. And further, if these hypotheses are to be like those about Smith then the question which of them is correct must not be settlable by indirect means either. That is, it mustn't be the case that the one hypothesis would lead us to expect one thing and the other something different. For example, though we may say, "If it's a brownie he wears a peaked cap", and thus deduce different consequences from the brownie hypothesis from those we deduce from the leprechaun hypothesis, we must not deduce, "If it's a brownie, he won't care for milk so it will be no good putting a saucer of milk in front of the watch in order to get it to go better; but if its a leprechaun it will be some good". For that would be a practical, a visible consequence. No practical consequence like that must be deducible from the one hypothesis which is not deducible from the other. Otherwise the leprechaun-brownie dispute will not be *idle*. It will be practical, like the hypothesis that Smith is colour blind *before* it reaches its philosophical condition. If in the ordinary way I say "Smith says he sees the

Union Jack in red, white and blue but I suspect he's colour blind and sees everything in greys " this is a serious suspicion. For if I am right we cannot take him on as an engine driver. But if when you say " Then we can't take him as an engine driver " I reply " Oh, he's all right in that way, in fact, do what you will, you'll find no sign of his weakness ", then my hypothesis has reached a philosophical condition. Likewise the dispute as to whether it is a leprechaun or a brownie who makes my watch sing may be either practical and settlable, or academic and idle. It becomes the latter when not only every tested but every testable consequence of the two hypotheses is the same.

12. *Don't say the dispute then becomes meaningless.* Don't say the question " The watch sings fairy songs, but now *is* there a leprechaun within ? " becomes meaningless. For then you will have to say that the question " The watch sings fairy songs, but now *is* there a brownie within ? " is meaningless. And yet here are two hypotheses, not one hypothesis.[1] And how could they be two if they were meaningless ? To imagine the one is one thing, to imagine the other another. And they have different consequences, only these different consequences are not observable, because the fairies are by nature invisible to human eyes.

To call the question which of these hypotheses is correct meaningless is too abusive. On the other hand, to call it idle is not abusive enough. It is an idle question how many grains of sand are in my hour glass and the question " Are there one thousand three hundred and seventy-six or one thousand three hundred and seventy-eight " means less to me than the question " Is it a brownie with a red cap and a white owl's feather or a leprechaun in green who runs my watch ? " or the question " Is Smith in pain or is he pleased when he grins and says ' Delicious ! ' ? " Nevertheless these last two questions are idle in a way in which the idle question about the grains of sand is not. If I ask you the question about the grains of sand I request of you an effort in a certain technique which will result in your being able to tell me the answer, or I ask you to tell me the result of past effort. But when I ask the question " Is it an invisible leprechaun or an invisible brownie which produces these phenomena ? Each would

[1] Compare the three hypotheses with regard to the man under hypnosis who, told that he will see no *t*'s, calls out ' Blank ' every time he comes to a *t* when he reads out the letters on a page of print. " Does the hypnosis alter his sensory apparatus so that he gets a sensation as of a blank ? Does hypnosis produce an eccentricity in the way he uses words so that though he says ' Blank ' he sees a *t* ? Does hypnosis turn him into a liar so that though he sees a *t* and knows it is called a ' *t* ' he consistently calls it a blank ? "

do so as readily as the other and there is no experiment which would tell us which it is ", then I don't ask you to set about anything. I am not asking a question. I make an interrogative noise which would usually act as a request to look for certain things. But by (1) the use of ' invisible ' I exclude some of these, and by (2) saying that the outward signs would be the same I exclude the rest.

13. *Degrees of departure from a difference in odds offered, to a picture preference.* Between disputes which reflect no issue suitable for betting, no difference of expectation, and disputes which reflect only difference of expectation, *i.e.* disputes which will be settled at once when a certain experiment is done, there are many different shades of dispute. In some the betting issue predominates, in others devotion to a notation with its picture. Only when there remains no experiment which will influence the disputants at all has the dispute become entirely of the latter sort, so that their assertions register not at all difference in expectations but only devotions to different working pictures.

14. *Equality of prediction with diversity of picture produces conflict as to equality of meaning.* Usually if two expressions differ in the pictures with which they are worked they differ in expectation value. And when two expressions differ in both these ways we have no hesitation in saying that they mean something different, and when they agree in both these ways that they mean the same. It is when expressions differ in one of these ways, picture say, and agree in the other, prediction say, that we get into difficulties. Shall we say that the brownie and leprechaun hypotheses are the same because to say that a watch is " brownie-driven " works on a hearer like saying that it is " leprechaun-driven " in the ways we have described ? Or shall we say that they are not the same because of the differences which we have described in the way they work on hearers ?

Does " the Sun is rising " mean the same as " This part of the earth is coming into the sun again " ? Does " The sea is angry " mean the same as " The waves are high " ? Does " At last a turn of the road hid them. He turned away. His eyes filled with tears ", mean the same as " At last a turn of the road hid them. He turned sadly away " ?

15. *The issue " Are there two hypotheses ? " put again.* To put the issue again : Suppose that amongst grandfather clocks some have been quite normal, and some have been abnormal and sung fairy songs on request and disappeared on Midsummer Night. On opening these, leprechauns have been found. Next, to everyone's surprise, some watches are discovered which sing

fairy songs and disappear on Midsummer Night. But of course the watches are too small for ordinary leprechauns and when they are opened no leprechauns are discovered. Someone suggests that nevertheless there are tiny, invisible leprechauns within (Puck in the churn, germs in the blood, agitated invisible particles in the table, a current in the wire) and that's why the watches behave curiously. This idea is generally accepted, but you, being philosophical, ask, " But *are* there leprechauns in the watches ? " What is it you are wondering ? You are not wondering what would happen if you dipped the watch in milk or left it under the moon. You've tried all that, you know all that. But then what are you wondering ? You are not wondering anything. " But ", you will say, " surely I *am* wondering something. I am wondering whether there is an invisible leprechaun in this watch—or a brownie, or nothing at all. The rival images are before me. Surely I am contemplating two alternatives, two possibilities ? "

To this I should reply, imitating, I hope correctly, Wittgenstein: " All right. If that's good enough for you it's good enough for me. Let's call what you do ' contemplating two possibilities'. Like you, when a leprechaun and a brownie are mentioned, I have two images ; and even if I don't, still, even from the first, when I hear someone say ' There's an invisible leprechaun in my watch ' I have some idea what he means. I expect there to be something queer about the watch and I am not surprised that it sings fairy songs, not hymns."

15.1. *Learning the meaning of ' absolutely invisible leprechaun '.* Perhaps I am a bit puzzled at first about the ' invisible '. Why does he say " There's a leprechaun in my watch " if there is no such thing to be seen or felt when it's opened ? However, as soon as he explains that his watch sings fairy songs, etc., then I quite understand when he calls to me, " And to-morrow I'll bring you a watch with an invisible leprechaun inside ". In just the same way I can come to quite understand if he says, " I'll bring you a watch with an invisible brownie inside ".

Although from the first I have *some* idea what a man means when he says, " There's an invisible leprechaun in my watch ", or " There's an invisible brownie in my watch ", I don't at first know how much he means, what his statement comes to. So at first my understanding is only partial compared with my understanding of his saying crossly, " There's a demon in my watch ". This last is different. I am used to this. I just smile. We all know what this comes to, we're very scientific nowadays and don't believe in demons. What he means is that his watch has

again let him down, although he's just had it put in perfect order, and so he's pretty cross. That's what his statement comes to.[1] But what his sober statements "There's an invisible leprechaun, there's an invisible brownie, in my watch" come to, that I don't quite know *until he has explained how his watch sings fairy songs and thus explained how much is left of the usual significance of 'There's a leprechaun in my watch', even when to it is added the destructive word 'invisible'.*

15.2. *The effect of regaining significance in this way.* But now if after explaining this way what he means by "There's an invisible leprechaun in this watch", "There's an invisible brownie in this watch", he then calls out as he leaves, "Shall I bring you a leprechaun-driven watch or a brownie-driven watch?" then I should be at a loss. I should say, "Well, but what's the difference?" He would reply, "One is driven by a leprechaun, the other by a brownie". I should say, "Yes, but as they are both invisible—what do you mean, which would I rather have?" "There's no difference in what the watches do", he would say, "but would you rather have a watch worked by a leprechaun or one worked by a brownie?". "Oh", I should say, "you choose", only now this would be a joke. Choose! How can he choose? Does he choose or doesn't he? Between the two alternatives, leprechaun, brownie, does he choose? What he does is to choose a watch which sings the fairy songs and then choose *whether to say* this is because it's run by a leprechaun or *to say* it's because it's run by a brownie. Maybe someone else, like those who prefer mnemic causation to invisible twists in the brain or unconscious images in the mind, will prefer to say, "The watch sings fairy songs, especially by moonlight, disappears on Midsummer Night, sings better if offered milk or honey. We don't know why, and it's no good pretending we do." Someone else again, instead of telling the long story about what the watch sings and when it sings best, will say, "It is *as if* this watch were possessed by a leprechaun or a brownie". Of this we readily say that it is just another way of describing how the watch behaves. If now someone says, "I believe there *is* a leprechaun within or a brownie", we say, "What do you mean? You know there isn't". He says, "An invisible one I mean". Mightn't we then say, "A hypothetical one you mean. You don't believe

[1] But is that what it means? A consideration of this case would be illuminating if only people would consider it seriously. But they say, "Ah, *that*—that's different. There's no puzzle about that, no one could find any difficulty there." Certainly it's different. Certainly *hardly* anyone would *for long* be *much* puzzled about it.

anything different from the rest of us. With you this is only another way of describing the watch." "No", he says, "I mean that there is something like a leprechaun, the presence of which *explains* the peculiar behaviour of the watch. I don't just mean that the watch behaves as it does. I believe that there is a reason why it behaves so queerly and I suggest that that reason is the presence of a leprechaun. You know what a leprechaun is like from the ones you've seen. Well this one is like those, only it's invisible. It is, of course, absurd to talk of *knowing* that it is there or *knowing directly* that it is there, because knowing directly means seeing or touching and it is self-contradictory for an invisible leprechaun to be seen and an intangible one touched. But what I say is there may *be* one there though we can't see it." "Ah", we should say, "we know now what this wondering comes to with you. You expect there's a stone in the hoof of a horse that suddenly goes lame. You don't in the same way expect there's a leprechaun in the watch that suddenly sings. You don't expect anything different from the rest of us, so don't try to excite us about yourself by describing yourself in a way which might lead us to suppose you mad. And you can call your hypotheses of a brownie and a leprechaun two different hypotheses, if you like. It doesn't matter you saying this because now we know what this amounts to, because now we've brought out what there is to make one say 'There are two different hypotheses'. There *are*, we have seen, differences between a person who speaks of the eccentricities of the watch as caused by a leprechaun and one who speaks of them as caused by a brownie. But we have also seen that these differences are such as to make very peculiar the question, 'Which of these people is right, which of these hypotheses is correct?' It is a peculiarity which came out in the way 'Shall I bring you a watch worked by a brownie or one worked by a leprechaun?' leaves us at a loss. The question leaves us at a loss in a way which is apt to make us say, 'The question is not a question. There aren't two possibilities. There's no difference. It's only a matter of which picture you paste on the back to remind you of the same peculiarities in the watch.'"

It's the same peculiarity of question which tempts one to say, "The philosophical scientist who suddenly doubts whether a current runs in wires which give off sparks, drive trains, etc., *isn't doubting anything*". Whether it is correct to say this is of as much and as little interest as whether unintentional promise keeping is promise keeping.

The question whether Jack yesterday kept his promise loses

all interest once the relevant features of the case have been described in " aseptic " [1] language. " One day Jack said to Dick, ' I will leave you all my money '. Next day Jack was dying and he said, ' I won't leave my money to Dick but to my next of kin, who is my wife '. So he wrote, ' All my property is to go to my next of kin '. But unknown to Jack his wife had died. And this left Dick his next of kin. Did Jack keep his promise or didn't he ? " The question is no longer of interest. Every philosophical question answers itself if asked aseptically and *fully* enough. And the question " Does the philosophical scientist doubt, does he not know which of two alternatives is correct ? " loses its anxiety-drive when, as we have done, we set out the conflicting forces, describe in aseptic language all the influencing features of the case.

16. *I want to explain again when and how " Is there a leprechaun within ? " isn't a question.* In the ordinary way, " There's a leprechaun in this watch " would lead us to expect to see something extraordinary. If a man explains that when he says such a thing we needn't expect to see anything of the sort, then when he says " There's a leprechaun in this watch " his remark works on us very like the first sentence of a story, " In the clock on the stairs lives a little old gnome. . . ." And we no more ask him " Are you *sure ?* I see nothing " than we ask this of the story-teller. One might be inclined to say, " The sentence now means little to us ". On the other hand, one might say, " It *means* just as much as before, only now we don't take it seriously ". If we speak the latter way we are apt to assume that it still makes just as much sense to ask whether the statement is true or false, it is just false. But is it ? Considering the speaker wasn't wishing to lead us to expect anything in particular was he speaking falsely ?

Suppose now that we are told that " There's a leprechaun within " isn't to be taken as mere fun, as a fairy story, that there really is something peculiar about leprechaun-driven watches, that they sing songs, disappear, etc., then the sentence begins again to work in a way which makes it not inappropriate to ask when someone utters it, " Are you sure ? ", " Is he right ? " But the appropriateness returns to these questions *only in so far as* what was taken from the significance of " There's a leprechaun within " by the story-teller is returned to it by the scientist—so far and no further. Consequently, " Is it true ? " is appropriate now only while it is still a question, What is its eccentric behaviour like ? Is it leprechaun-like or isn't it ? "

[1] See note to page 369.

17. *There are two cases where "Is this watch's behaviour leprechaun-like?" no longer puts a question and in which consequently "Is there a leprechaun in this watch?" no longer puts a question.* The first case is where it would ordinarily be said to put a question but the question is a conflict question and has no answer. I want to explain why I say that this question is not a question, in order to help explain why I say in the second case that "Is there a leprechaun within?" no longer puts a question. The second case is one in which a man who is asking "Is this watch's behaviour leprechaun-like?" is no longer putting a question because it has been fully answered. In this case "Is there a leprechaun within?" puts a question only in so far as there is a jump between criteria and that of which they are criteria.

As to the conflict case. It may be that the behaviour of a watch is in many ways leprechaun-like but in as many ways not. Then, if asked "Is there a leprechaun within?" it would be quite usual to reply, "I hardly know". But this ignorance isn't ignorance, this doubt isn't doubt. One begins to notice the peculiarity of this ignorance when one notices its irremediableness, its logical irremediableness, how it is combined with the most perfect knowledge of, and confidence in, the watch. And then one notices how instead of replying "I hardly know" one might have replied "I hardly know what to say", and how again instead of this, one might have replied "You ask 'Is the behaviour of this watch leprechaun-like?' Well, it is and it isn't". And then, if one feels a bit of a qualm at saying that, one may notice the connexion between "Is its behaviour leprechaun-like?" and "Are its behaviours, its reactions, leprechaun-like?". Directly we have the question in the plural like this we no longer need to reply "I don't know", "It is and it isn't", because we can answer, "Some are and some aren't".[1] And we may add, "And whether there are enough of its reactions which are leprechaun-like to call its behaviour

[1] This is an extremely simple case of the effectiveness of the Verification Principle. The verification for "Is its behaviour leprechaun-like?" is "Are its reactions leprechaun-like?" The illuminating effect of this translation is seen in the text. Dr. Dorothy Wrinch once pointed out how the question "Does he represent the feeling of his constituency?", which is all right when the people of the constituency "feel as one" but becomes embarrassing when some feel one way and some another, can be translated into a form which is not embarrassing in either case, namely, "Do the persons in his constituency feel as he does?" In this way she provided an excellent example of the importance of logical constructions, levels of language and the verification principle.

leprechaun-like is a matter for decision ". The anxious doubt has become a cheerful indecision ; the request for information that no expert in heaven or earth can provide has become a request for a decision, a judicious decision though.

Instead of saying " Is its behaviour leprechaun-like " isn't a question and doesn't express a doubt because it turns into indecision and choice, we might say " Is its behaviour leprechaun-like ? " expresses a doubt of a sort very *peculiar* in that it is capable of turning into indecision and choice. And this has an advantage, because the transformation from a state which at first is like doubt to a state which is like indecision [1] as to whom to award the prize to, is a very important transformation.

And just as the doubt from conflict of criteria about whether the watch's behaviour is leprechaun-like is not a doubt because it turns into indecision, so the doubt from conflict of criteria as to whether the watch is leprechaun-driven is not a doubt because it turns into indecision. Or, if you like, it is a doubt with the peculiar property of turning into an indecision. We may say that conflict-doubts *dissolve* into indecision—to use Wittgenstein's word.

18. *It is for the same sort of reason, namely the way they dissolve, that I call doubts when the criteria do not conflict but are all for " Yes " or all for " No ", not real doubts. For they dissolve. Not into indecision. But into decision.* One who says " This wire gives off sparks, drives trains, rings bells, etc., but I doubt whether there is a current running through it " is declaring against saying that " gives off sparks, drives trains, rings bells, etc." entails " has an electric current running through it ". This is why it would be absurd for anyone to say, " This wire if attached to a battery at one end and a bell at the other will ring the bell, etc., but I doubt whether it behaves *as if* a current runs through it ". For " will ring a bell when suitably attached, etc." obviously entails " behaves *as if* a current runs through it ". But one who says, " All the same I doubt whether there *is* a current ", *is he absurd or isn't he ?* Do the features mentioned entail " There is an electric current within " ? When he says " Accepting $s_1 \ldots s_n$ I can yet doubt whether S is P " what he does is to say that $s_1 \ldots s_n$ do not entail S is P. When a man says " $s_1 \ldots s_n$ are true but is SP ? " then implicitly he denies that $s_1 \ldots s_n$ entail S is P.[2] True, there are cases where this isn't all he is doing, *e.g.* A man in the dark says, " This has hands, face, and feet but is it a man or a mummy ? " But, on the other hand, there are cases where this *is* all he is doing. My point is that in

[1] Or doubt ! as we might also say.

[2] See note 1 to page 369.

philosophical cases this is all he's doing, that he isn't besides asking whether S is P. To grasp how philosophy though not logic is *a priori* and though *a priori* is not logic takes one far towards dissolving its difficulties.

Let us return to " Is there an invisible leprechaun in this watch ? " where the scientist has given it back some of the meaning the story-teller took from it. We said that after the scientist had explained the meaning he gave to " There is an invisible leprechaun in this watch " it could yield a question only while " Is this watch's behaviour leprechaun-like ? " was a question. And we said that there are two cases where this becomes no longer a question. They are of course two cases where all is already known about the behaviour of the watch. The first case, which we have now noticed, is that in which some of this complete information favours an affirmative answer and some a negative. The second case, which we have now come to, is that in which either all of this complete information favours a negative answer or all of it favours an affirmative answer. In this case, if a scientist who has told us that we needn't expect to see a leprechaun when he speaks of a leprechaun-haunted watch but need expect only what we have found, then asks " But is this watch inhabited by a leprechaun ? " we are at a loss.

Someone may say : " Of course if the scientist has explained that all that he *means* by " Is there a leprechaun within this watch ? " is " Does this watch behave as if there were a leprechaun within ? " then if, knowing that it does behave so, he asks " But is there a leprechaun within ? " his question is absurd. But if that is not all that the scientist means then his question is not absurd. When the housewife says " Puck's in the churn " she doesn't mean merely " The churn behaves *as if* a small ill-disposed person were in it ". And the scientist who says that through wires giving electrical phenomena there runs a current, isn't merely saying that wires giving electrical phenomena give electrical phenomena. Much less is it the case—if this is what you should be driving at—that Smith sees the Union Jack in red, white, and blue just means " Smith acts as if he does ".

I must protest that this is the point at issue. Those who say, " I can still doubt whether Smith sees the Union Jack in red, white and blue however normally he behaves," sometimes put their position thus : " ' Smith sees the Union Jack in red, white and blue ' doesn't *mean merely* that he says that it's red, white and blue and points to roses, snow and the sky, etc." Since the point is sometimes put this way, I must insist that to say " Some-

times the scientist doesn't mean merely 'behaves as if, etc.'" is to claim the point at issue.

However, by all means let us consider people who speak of currents and leprechauns without explaining in so many words that all they mean by "A current runs here" is "It is as if a current runs here", and that all they mean by "A leprechaun lives here" is "It is as if a leprechaun lives here".

There is something about these people which makes us say "They mean an *invisible* current, an *invisible* leprechaun" just as there is something about the man who says "Poor Smith, he always has in his head the picture of the motor smash in which his brother was killed" makes us say, "He doesn't mean a visible picture to be found by cutting Smith's head open".

What makes us say the scientist means an invisible current? It is the fact that, like the scientist who did say in so many words that all he meant was "behaves as if", this scientist whom we are now considering does not expect to see a flowing stream in the wire, not even with the strongest microscope, and doesn't wish you to expect this when he tells you that that wire carries a current. He is in fact just like the scientist who said in so many words, "All I mean by 'an invisible current' is 'behaves as if there is a current'", except that (1) he didn't *say* that, and (2) he is much more likely to confuse himself or be confused by others with the question "But *is* there an invisible leprechaun within?" But if he didn't expect to see anything why should his seeing nothing suddenly arouse his suspicions? His suspicions of what? What that he formerly had confidence in has he confidence in no longer? Not the seeing of sparks, for he is still confident of that. Not seeing a stream, for he never was confident of that. So if he now says "A doubt has struck me", then it's a queer doubt since it involves no loss of confidence in what formerly he had confidence in. As to whether he *means the same as* the former scientist who actually *said* "I mean by 'There's an invisible current within' 'It's as if there were a current within'", I must leave you to *decide*.

The very fact that this latter scientist asks: "The sparks fly, etc., but is there a current within?" might be taken to prove that the answer to his question is "No". For since he asks "Is there a current" where he doesn't ask "Do sparks fly, etc.?" it follows that he uses these expressions differently, *i.e.* that they mean something different. And if they mean something different, then the flying sparks don't *prove* the pressure of the current. Speaking this way, one will say that all philosophical questions of the form "Does S mean S′?", "Do I know S from S′?"

answer themselves in the negative. But I don't myself wish to decide as to whether the scientist means by " There is a current in this wire " the same as he means by " This wire gives off sparks, etc.".

19. *In other words : what I want to insist upon is how* peculiar *it is when he suddenly says " I wonder if there is an invisible current in this wire "*. He is like someone who suddenly says, " Is there such a thing as a wind ? I see nothing except its effects (*sic*), the hurrying clouds, the flying leaves ". And one who suddenly doubts, saying, " I wonder whether in gold, perhaps, a negative current produces all the phenomena we associate with a positive current ? " is like one who suddenly doubts, saying, " I wonder. Perhaps on Sundays when sailing boats fly eastwards, the wind is not, as we commonly suppose, from west to east, but from east to west ; because perhaps on Sundays winds have opposite effects on things in the wind from what they have on other days of the week ". And now consider one who says, " Who knows ? maybe with mad people signs of melancholy are all produced by happiness. Maybe with Smith signs of seeing red are produced by seeing black ".

One might say, " These fancies aren't absurd. It's only absurd to be serious about them, but that not at all because they contemplate *very* remote possibilities. They don't."

Someone may say, " Though we can't say what one who believes in fairies or gods which can't be seen and never act except in accordance with the laws of nature, loses confidence in when he says ' Maybe you are right and there are no spirits in the trees ', still he does seem to lose confidence in something. An empty picture rises and his heart falls, very like it happens with one who really believes in visible fairies and hoped to show them to you beside the lake but now suddenly doubts whether they have gone and won't return."

I said with regard to the question " Does Smith, who gives all signs of believing that flowers feel, really believe this ? " that the qualm it brings is like the qualm that goes with a real doubt. It is the same with the qualm and picture that goes with " But are there invisible fairies ? " It too is like the qualm that goes with a real doubt, such as " I wonder, *are* there any orchids in this wood ". The questions " *Is* there an invisible leprechaun in this watch ? ", " *Is* there an invisible current in electric wires ? " both come with an empty picture and an empty feeling, like " Maybe there are no orchids now ". But what we have seen is how different nevertheless are these mock doubts from betting doubts. A father who pretends he doesn't know what is in the

parcel his child has given him, feels as if he doesn't know, feels as if he is guessing, feels as if he thought it might be alive and bite him. But he doesn't really think it might be alive, he really knows that it's chocolate, he's only pretending to wonder what it is. But since he *pretends to himself* that he doesn't know *is* he only pretending? Shall we say that for the moment he forgets he knows, that for the moment he wonders? When he turns quickly to fire on the wolves who are gaining in spite of the efforts of two exhausted rocking chairs, he is really excited. He is pretending that wolves are behind but he isn't pretending he thinks that wolves are behind. He feels they are, he sort of believes they are, he half believes it. No, it isn't that he half believes it, he doesn't. It is more that just for the moment he believes it. The illusion is fragile. It breaks if you touch it. At least it does if you touch it with an unsympathetic hand. If you ask "How much further to the Inn?" that won't break it. But if you say "There are no wolves nowadays", then the father will say "We meant pretence-wolves". He is like the scientist who says he meant an invisible current, so invisible that not even stronger microscopes need be expected to reveal it.

20. *Everyone can see that it's silly to ask, "But are there any pretence wolves?"* If anyone asks this he is making an academic joke like one who says, "Since you passed nobody on the road, nobody walks slower than you".[1] *But people don't always laugh if you talk as follows: "But* IS *there an invisible current in wires giving electrical phenomena?* We *say* that an electric current runs with less resistance through steel than through rubber, but all we *know* is that the phenomena at the other end are more marked. Perhaps the current is as strong, only in rubber it shows itself less." And serious economists write articles round the text that we don't know what amounts of pleasure different people get from things. They write, "We say that Smith got more pleasure from his last and second glass of beer than Jones got from his last and tenth glass, but all we know is that Smith would have been prepared to work longer to avoid losing it than would Jones, or that Smith would have given a greater proportion of his possessions to avoid losing it. But maybe his other possessions gave less pleasure to Jones. We know nothing about the absolute amounts of pleasure things give to other people, we only know the ratios in which they prefer them. When an economist, as such, talks of a thing giving more pleasure to one man than to another, this is only *a way of talking about* what each would give for the thing. The economist should, strictly speaking,

[1] Adapted from *Alice Through the Looking Glass.*

answer themselves in the negative. But I don't myself wish to decide as to whether the scientist means by " There is a current in this wire " the same as he means by " This wire gives off sparks, etc.".

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I said with regard to the question " Does Smith, who gives all signs of believing that flowers feel, really believe this ? " that the qualm it brings is like the qualm that goes with a real doubt. It is the same with the qualm and picture that goes with " But are there invisible fairies ? " It too is like the qualm that goes with a real doubt, such as " I wonder, *are* there any orchids in this wood ". The questions " *Is* there an invisible leprechaun in this watch ? ", " *Is* there an invisible current in electric wires ? " both come with an empty picture and an empty feeling, like " Maybe there are no orchids now ". But what we have seen is how different nevertheless are these mock doubts from betting doubts. A father who pretends he doesn't know what is in the

parcel his child has given him, feels as if he doesn't know, feels as if he is guessing, feels as if he thought it might be alive and bite him. But he doesn't really think it might be alive, he really knows that it's chocolate, he's only pretending to wonder what it is. But since he *pretends to himself* that he doesn't know *is* he only pretending? Shall we say that for the moment he forgets he knows, that for the moment he wonders? When he turns quickly to fire on the wolves who are gaining in spite of the efforts of two exhausted rocking chairs, he is really excited. He is pretending that wolves are behind but he isn't pretending he thinks that wolves are behind. He feels they are, he sort of believes they are, he half believes it. No, it isn't that he half believes it, he doesn't. It is more that just for the moment he believes it. The illusion is fragile. It breaks if you touch it. At least it does if you touch it with an unsympathetic hand. If you ask "How much further to the Inn?" that won't break it. But if you say "There are no wolves nowadays", then the father will say "We meant pretence-wolves". He is like the scientist who says he meant an invisible current, so invisible that not even stronger microscopes need be expected to reveal it.

20. *Everyone can see that it's silly to ask, "But are there any pretence wolves?"* If anyone asks this he is making an academic joke like one who says, "Since you passed nobody on the road, nobody walks slower than you".[1] *But people don't always laugh if you talk as follows: "But* IS *there an invisible current in wires giving electrical phenomena?* We *say* that an electric current runs with less resistance through steel than through rubber, but all we *know* is that the phenomena at the other end are more marked. Perhaps the current is as strong, only in rubber it shows itself less." And serious economists write articles round the text that we don't know what amounts of pleasure different people get from things. They write, "We say that Smith got more pleasure from his last and second glass of beer than Jones got from his last and tenth glass, but all we know is that Smith would have been prepared to work longer to avoid losing it than would Jones, or that Smith would have given a greater proportion of his possessions to avoid losing it. But maybe his other possessions gave less pleasure to Jones. We know nothing about the absolute amounts of pleasure things give to other people, we only know the ratios in which they prefer them. When an economist, as such, talks of a thing giving more pleasure to one man than to another, this is only *a way of talking about* what each would give for the thing. The economist should, strictly speaking,

[1] Adapted from *Alice Through the Looking Glass.*

define consumer's surplus in terms not of pleasure but of the ratio of what a man would give for a thing to what he has to give for a thing. The question whether a poor man gets more pleasure from his last pound is, strictly speaking, a psychological or metaphysical question rather than a question of economics, though it is often convenient to talk in absolute terms of amount of pleasure and desire, just as it is often convenient to talk in terms of amounts of energy almost as if it were a subtile fluid, when really all we know is that coal will keep the room warm longer or drive an engine further than the same bulk of wood."

21. *Well now, why is it that though few people will take seriously*[1] *the question "But are there pretence wolves?" many will take seriously "But are there invisible electric currents?" and many would take seriously "But are there invisible leprechauns?"* One way of answering this is to say: It is much more obvious that pretence wolves are not a species of wolf than it is that invisible leprechauns or invisible wolves are not species of leprechaun or wolf, *i.e.* it is so much more obvious that the grammar of 'pretence wolf' differs from that of 'curly wolf' than it is that the grammar of 'invisible wolf' differs from that of 'curly wolf', *i.e.* it is so much more obvious that questions which make sense with 'curly wolf' and 'wolf' may make no sense with 'pretence wolf' than it is that questions which make sense with 'curly wolf' and 'wolf' may make no sense with 'invisible wolf'. And this is partly because there *is* more difference between the grammar of 'curly wolf' and 'pretence wolf' than there is between the grammar of 'curly wolf' and 'invisible wolf'. But it is also partly because there are uses of 'invisible wolf' which are not so very different in grammar from the grammar of 'curly wolf' and *these uses are not sharply separated from the use in which the grammar is very different.*

In other words: People are more inclined to take seriously the question "But are there invisible electric currents?"[2]

[1] The *busy* scientist is impatient of this question because he knows that "what he wants to mean" when he talks of invisible currents is certainly true.

[2] Compare the seriousness with which people take "But is there such a thing as force or stresses and strains apart from our own sensations of strain?" and "Is there such a thing as sensation which I don't feel?" The seriousness with which people take this is not unconnected with the seriousness on some occasions of questions of the form "Is there an invisible X?" But it has further sources which will be brought out in the next instalment, where the point is taken up. But "Are there fairies in the trees" is different from "Are their souls in bodies?". "Are there invisible twists in the brain?" is different from "Are there unconscious events in the mind?"

because very often when someone says "There's an invisible so and so here" it's not silly to ask "Are you sure?" For example, "Is there invisible ink on this paper?" It is not silly to ask "Is there an invisible X here?" (1) when though X's can't be seen with the naked eye they can be seen with microscopes or after prayer and fasting; it's not silly (2) when though there is *no* way of seeing an X there remain tests for an X which have not yet been carried out. The question becomes silly only when neither (1) nor (2) is the case.

It may be said: "There are no cases where there is no further test to be carried out". There is no need to dispute this. My point is that (1) there are cases where the source of dispute is not at all these uncarried out tests, and that (2) these cases are not sharply separated from cases where the dispute does arise from different expectations, but are joined to them by cases where the dispute is partly empirical and partly *a priori*. Thus when one doctor disputes with another as to whether a child who coughs and vomits but doesn't whoop has whooping cough, the source of the dispute is largely a matter of whether one should, in view of the absence of the whoop, speak of whooping cough. This is an *a priori* issue. But that there is this *a priori* difference between the doctors is obscured by the fact that the dispute probably also *in part* arises from different expectations as to the course the illness will take and how it will respond to this treatment or that. It is obscured, too, by the fact that the forms of words, "It is whooping cough", "It isn't", always can at any stage in the dispute be used to express an empirical dispute, since it is quite true that there always remains some uncarried out test, with the consequence that it is always possible to pretend that it was an empirical issue one had in mind in raising the question.

The dispute is nearly always purely *a priori* when the question asked is not particular, not, for example, "Does this wire carry a current?" but general, for example, "Can we *ever* be sure that *any* wire carries a current?" Such a question usually arises not at all from the incompleteness of our researches into the habits of, *e.g.*, wires, and so can be put in the form, "Even if all the infinite number of tests for, *e.g.*, a wire carrying a current had been carried out and found favourable to its carrying one, should we know that it carried one? Would it follow that it carried one?" In this form the *a priori* character of the general question has become apparent.

21.1. *The uses of "Is this leprechaun-driven?"* Let us remember then (1) that the particular question "Is there an

invisible leprechaun within this watch ? " may be purely empirical in that it is asked because, though from the watch's face the questioner thinks it's leprechaun-haunted and will sing if asked, he isn't sure ; (2) that nevertheless the question may be asked *partly* because the questioner doesn't know what the watch is capable of but also *partly* because he isn't sure whether it is proper to call a watch leprechaun-haunted when it sings fairy songs all night but only when asked and never of itself. The more the question arises from the latter source the more *a priori* it is.

Now comes a second point. The field of uses of " Is this watch leprechaun-driven ? " has another dimension beside that running from the empirical to the *a priori*. For at any point on this scale the questions differ according to whether they arise because the questioner doesn't know the usage of the expressions involved in his question as well as he might or not because of this. Thus, suppose he asks because, though the usage of ' leprechaun-driven ' is *in fact* fixed so that we don't call a watch ' leprechaun-driven ' but ' parrot-driven ' when it sings fairy songs only after it has just heard them performed, *the questioner doesn't know this*. Then we can put him right, tell him the answer to his question. Then our conversation with him will be a conversation in logic. But it may be that he doesn't know whether to call leprechaun-driven a watch which sings songs only when it has just heard them, not because there is a convention on this matter of which convention he is ignorant, but *because there is no convention on this matter*. There may be no customary procedure for such a case either because no such case has before arisen or because when such cases arise some people do one thing and some another. The more indefinite custom is, the less is the question a matter of logic and the more it calls for decision. There are two cases : (*a*) where the questioner asks the question as a logical question, that is, expecting us to be able to tell him the answer by improving what he thinks his defective knowledge of when it is " the done thing " to call a watch leprechaun-driven ; (*b*) where the questioner is amused and expects us to be puzzled as to how to help him because he knows he knows as much as anyone about when it is the done thing to call a watch leprechaun-driven.

Finally, we come to a queer case where the questioner knows what the usage is and the usage is definitely for (or against) saying that a watch is leprechaun-driven. This is the case we have been considering. What in such a case induces the questioner to ask his question ?

A man touches a wire, asking, " Is there any current in this ? "

and as he touches it there's a blaze of sparks, a crackling, and a spasm crosses his face from the shock he gets. A man watching says, "Is there a current?" but his question is a joke, an exclamation.

It's a joint joke because the speaker and hearer realise the "inappropriateness" of the interrogative form.

21.2. *When a philosopher under the same circumstances doesn't laugh* but insists on seriousness *and says, "Ah, but is there any current there?* Of course it gives off sparks and so on but how do you know from this that there is a current there?" *then what is he up to?* He is not issuing an empirical warning like "True it looks like a log. But isn't it an alligator?". Nor a logical warning like "He published abroad the scandalous story, but that wasn't libel, because the story was true".

The philosopher who says "Do we, even when we know that a wire gives all electrical symptoms, know that an invisible current runs in it?" is more like a man who when one says "I'm afraid Brown is mad" says "Aren't we all mad really?". This looks like an empirical warning such as the vet gives when having just examined a lot of horses he says "They all look sound. But are they?" and pulls down his mouth in a knowing way. Nor is it a logical warning like "These horses seem sound but, don't forget, you oughtn't to warrant any of them without a trial on hard ground for navicular disease". But "Aren't we all mad really?" and especially "Aren't we all mad really, if you come to think of it?" is often said without expecting anything different of people from what all the rest of us expect. And it certainly isn't the case that we oughtn't to call a person sane without trying the effects of a week's solitary confinement on him. So no one ought to say "Are any of us sane? *Don't forget* you oughtn't to call a man sane until you have tried solitary confinement on him". On the contrary, the person who says "Aren't we all mad, if you come to think of it?" is suggesting a logical reform. He wishes to draw the line less sharply between the mad and the sane, like one who says "We are all sinners". One who says "We are all sinners" ("We all have an Œdipus complex") *may* say this because from special experience he is confident that even those who best keep the ten commandments in Kensington would react in other places in such a way that we should all say "They were sinners at heart all the time". I say he *may* say it because of this. But he may say it not at all because of this. He may say it because though he would in the ordinary way entirely agree with us as to who is good and who is bad and on what they may be expected to do, nevertheless

reflecting on the life of the pious Mr. Jones and the wicked Mr. Vincent who ran off with Mrs. Jones, he "*feels there isn't such a difference between them*" and feels disinclined to give the one a white label and the other a black. He suggests a new notation which shall make a difference between the difference between sheep and goats and the difference between saints and sinners.

It is true that often these "wise" generalisations, while they arise partly by reflexion in the way I have just reminded you of, come partly out of experience, and when this is so then, while partly they are recommending a notational reform, partly also they are issuing an empirical and logical warning so that their empirical and logical air is not entirely misleading.

Take the man who says "Aren't we all mad really?" Surely, often he is wishing to make our reactions, our confidences, more continuous, so that we no longer have *no* confidence in the mad and such great confidence in the unmad. He wishes to alter our expectations of the mad and the sane as one who teaches us a thing or two about doctors, clergymen and teachers. So isn't he issuing an empirical warning? And as it is a warning about not taking certain appearances too unquestioningly, isn't it a logical warning as to the estimates we put on the evidences we get? But can it be both a logical and an empirical warning?

As to the last point. When a man says "Bill isn't in the bedrooms and isn't in the sitting-room or the kitchen so he isn't in" then he is making an empirical statement and a logical one. And if another man says "It doesn't follow he isn't in. He may be in the cellar" then he is holding up his hand in warning both logically and empirically.

As to whether a man who says "Aren't we all mad really?" is issuing an empirical warning, there are many ways in which his question can be used. All can be described as uses in which the speaker wishes to draw the line less sharply between the mad and the sane. But this "drawing the line less sharply" can be anything between a feeling, on reflecting over what we all know about our own hearts and our friends', that there's not such a difference between us and the mad and a real belief that everybody has now gone mad, that everybody is mad, though at the moment everyone is quiet. Of course in the last case there would be nothing misleading about bringing out this generalisation with an instructive air. But no one uses it like this. People do, however, use it like "We are all sinners" or "We are all selfish", because, though they don't want to make the empirical generalisation "properly" expressed by "We are *all* mad", they do want, because of their disillusioning experiences,

to say that *some* or *many* people we think sane probably aren't.

But there is a more subtle way in which " Aren't we all mad ? " may reflect our eccentricity of expectation. A man may entirely agree with us as to who is mad and who is not, and yet his saying " Aren't we all mad really ? " reflect a difference in his expectations as well as attitude. Most people, if they find that one of their relatives has become very eccentric, will ask a doctor, " Is he mad ? " and if the doctor says " No, he's not mad " then they are entirely relieved and say, " No, he's only a bit eccentric, the doctor says he's not mad ". (" The doctor says " they say, with pathetic faith. But does the doctor know as well as they do ?) On the other hand, if the doctor says " He's mad ", they are plunged into despair, and suppose that now he must be hurried to a lunatic asylum. They look at him with scared and curious eyes. In contrast to this, the man who says " We are all mad " (all are sinners) says to the doctor, " You mean he's a bit ' madder ' than the rest of us or mad in more awkward ways ". And his saying this, though it may not reflect any difference in his bets as to who is mad and who isn't, may well reflect a difference in the bets he would make on the patient's recovery or on some sane person's doing this or that mad thing. His bets on various eccentricities in people will not show that sharp discontinuity between bets on eccentricities in the mad and bets on eccentricities in the sane, which the bets of ordinary people would show.

His expectations of eccentricity may be unusual in this way, although his devotion in ordinary life to the classification into sane and insane may be more stable than that of the rest of us. Most of us are apt if we find someone frightened into hysterics by the quack of a duck or unable to remember what happened five minutes ago, to exclaim, " He's mad, you'd never have thought it but he's mad ". The man of more experience has had experience of eccentricities in the sane and realises that since seven people out of ten are monsters they can't be.[1]

Of course to say " We are all mad " is a way of expressing his unusual expectations of eccentricity which is very apt to mislead. For people are very apt to suppose that one who says this at least believes that many people who are thought sane aren't. The pear expert who says " These pears look good, don't they ? But really they're all rotten at the core " has expectations of every pear very different from the rest of us, who are taken in by them. But the expectations of one who says " We're all mad really, deep down, only we keep it under " are not related to

[1] With acknowledgements to Henri de Montherlant.

his words in the ordinary way, they are not as eccentric as they seem. It is true that such a remark seldom expresses no eccentricity of expectation, that case is a limiting case. But though usually when a man says "We are all mad deep down" he's rather more prepared than the rest of us for poor Mr. Williams to brain his baby with its own bottle, usually he says what he says very largely because certain things we *all* know already but hurry over in our minds, can't so well be hurried over if we say "We're all mad deep down", "All sinners", "All selfish". And the more he's playing this literary game and not a scientific game the more does his statement lead up the garden path those who take all statements in prose as prose.

In so far as one who says "Aren't we all mad really?" realises how far he's not correcting our ordinary confidences and conclusions, so far his anxious and interrogative air is a conscious joke; in so far as he doesn't realise this the anxious and interrogative air is an unconscious joke, as when people feel they can't find out whether a tomato is a fruit or a vegetable, though here the inappropriateness of the interrogative air arises in a rather different way. I am not saying that the question "Aren't we all mad really?" is a joke. On the contrary, even the interrogative air is not a joke in so far as ordinary confidences are being questioned. But apart from this, the question has an importance of its own sort as we have seen. Still, it's something of a joke a man's saying, "We're all mad really", and making us open our eyes in wonder and fear when really we ought to nod our heads in a knowing way and screw up our faces with a bit of a shudder or a grin. He isn't denying that we are not all mad. And if he's telling us that the sane aren't separated from the insane like sheep from goats, then we knew that before. Still, we must hand this to him, he does bring it home, especially as and when he "supports" his statement.

And if a man says "We never know that there's an electric current in a wire whatever impressive phenomena it exhibits. We always infer, guess its presence", then we must hand him this: He brings home the fact that we can never know of the presence of an invisible current in a wire in the way that we can know of the presence of petrol in a pipe, that is, not merely from the behaviour of the machine but also by cutting a section from the pipe and taking a look. In this way our knowledge of the presence of an invisible current resembles our knowledge of the presence of petrol before we have looked. But to say that we don't know, we only guess, that there's an invisible current in an electric wire, is something of a joke. For our knowing that

there's an invisible current in a wire is very different from our knowing, our guessing that there's petrol in a pipe. In particular, the knowing that there's petrol in the pipe leaves open other hypotheses in a way in which the knowing that there's an invisible current in the wire does not, although, as we have seen, it does in its own way.

22. *And now to try to remember all at once this long description of the doubt "Surely from outward signs, however many and consistent, we cannot know a man's inward state, nor that his soul is still in his body, nor that it's not".* The Sceptic said: "Sometimes I feel we never know what is going on in the mind of another. Sometimes I wonder whether other people aren't just automata? Do you ever have that feeling? After all, how do we know, even when a man has given every sign of being most religious all his life, that he wasn't secretly quite the reverse?"

I said: "I know what you mean. But this doubt of ours is a queer one. To be plain, it's a philosophical doubt, and one never feels quite confident whether these are tremendously profound or quite silly. Now with this one, here we are pretending to doubt what the multitude unthinkingly assume. All the same, we expect nothing different from what they expect, all they look for we look for. What *are* we doubting?"

The Sceptic said: "Yes, it's hardly a doubt, it's the contemplating of a possibility".

I said: "Well it's more than that with me. When I feel as if I can't be sure whether there are any feelings behind people's faces I feel as though a big bit of the world which I had thought to be there though I couldn't see it, perhaps after all isn't there. And then, on the other hand, I feel a difficulty about saying that I am doing as much as the contemplating of a possibility. With the contemplating of any other possibility, such as Smith's being really colour blind though he passed the test against it, I am always contemplating what further investigation would reveal. But when I am contemplating the possibility of Smith's being colour blind although all investigation would give results to the contrary, then what am I doing?"

The Sceptic said: "Surely you can *imagine* that Smith, though outwardly perfectly normal, sees the Union Jack in black, green and yellow instead of red, white and blue".

I said: "I know what you mean. But now it is our imagining that is queer. Other imagining that what we had thought to be so isn't really so consists in imagining further investigation producing disappointment. But in this imagining that things aren't what they seem we imagine no disappointments for ourselves.

The image in this imagining doesn't guide to a copy of itself. When we imagine that there's a visible leprechaun in our watch we have a leprechaun-image which guides to a copy of itself, and whether what we imagined is so is determined by whether we find what we are guided to or not. But the image in imagining that Smith sees the Union Jack in black, green and yellow is an image of a black, green and yellow Union Jack which guides not to a sensation like itself but to Smith's painting the Union Jack in black, green and yellow when asked to paint what it looks like to him, and to his matching it with ebony, grass, and lemons instead of the usual pillarbox, snow, and sky ; so when by the terms of the philosophical imagining the image isn't allowed to guide to these things, the question " Is what we are imagining really so ? " hasn't answers. Imagining that Smith acts quite normally but not because his inward state is that of seeing the Union Jack in red, white and blue but that of seeing it in black, green and yellow, is like imagining that a watch's behaviour is due not to an *invisible* leprechaun but to an *invisible* brownie, that light is not corpuscular but wave-like. Here are rival pictures, and if you like rival hypotheses, but the pictures play a part in the hypotheses which makes it a queer question to ask " Which hypothesis is right " unless one *makes a story of* the facts to be explained better than does the other. When they do this equally well, the question " Which is correct ? " becomes inappropriate.

This is obscured by the fact that the question reaches this condition gradually. We often use " There's an invisible X here ", " There's an invisible Y here " in such a way that the question " Which is present ? " is not a joke, and these uses are not sharply separated from the use in which such a question is a joke because it disguises the request " Which fairy tale serves best to remind us of the clusters of phenomena in nature ? " Indeed, we may say, if we like, that the question never becomes *merely* this.

The peculiarity of the question, " Is there a leprechaun here or a brownie ? " when ' leprechaun ' and ' brownie ' are so used that both are absolutely invisible and intangible and both have just the same effects on watches, comes out, if we imagine someone taught this use and then asked, " Which will you have, a brownie-driven watch or a leprechaun-driven one ? "

Whether we say of someone who asks of a textbook case of a leprechaun-driven watch " *Is* there an invisible leprechaun here ? " that he is asking a joking question or making a joke, is a matter of choice, though the peculiarities of what he is doing are better brought out by saying that he is not asking a question.

To say this removes confusion as to what he is doing, prevents abortive efforts to meet his demands, in the same way that to say of someone who asks " Can one keep a promise unintentionally ? " that he is not asking a question, prevents abortive efforts to satisfy him. And this is so although what makes the one not a question is different from what makes the other not a question.

The promise-question dissolves into " Is it in a case exhibiting features $s_1 \ldots s_{n-m}$ but not $s_{n-m} \ldots s_n$ proper to speak of promise-keeping, where the usage of ' promise-keeping ' is not definite for such a case ". The dissolution of the latter and more characteristically philosophical doubt with regard to a text-book case of a leprechaun-haunted watch or, say, a wire with an electric current in it, " Is there a current in this ? " is more peculiar. It dissolves into " Given that the history, present condition and future of this wire is just such as leads us all to speak of the presence of an invisible current in the wire, are we right in so speaking ? " Here by hypothesis usage is definite. The consequence is that even in this form it is a puzzle to know what anyone who asks this question is up to. He appears to be asking in a doubtful voice " Ought we to say that we know q from p ? " in a case where he knows we all should say this. The only excuse he has is that we all feel some inclination to reform logic in the way he is suggesting. Looking at other cases of such astonishing temptations to departure from ordinary usage, such as " We are all mad really ", " We never know but what we aren't suffering from illusions ", we find that though these statements to some extent present us with the results of experience (the surprising eccentricities of the sane, the clever imitations at Tussaud's) they are often more a matter of *mentioning* certain matters which we know of but don't mention. These matters are not at all what is *literally* expressed by those daring departures from ordinary usage, whether in the form " All S is P " or in the form " No S is P ", which are used for drawing attention to them. These " wise " generalisations are thus extremely misleading to that sort of person who, however wildly one says " Women are cats ", " Men are pigs ", anticipates that one will support one's statement with statistics. These people will either think the philosopher quite absurd or they will be worried to death because while they cannot without a qualm reject what the philosopher says they cannot accept consequences which it seems to them it must imply. They will ask " Is he right ? " with an anxious and interrogative air which would amuse the philosopher if he weren't himself half taken in by his own imitation of a scientist.

When a man says "This car's possessed this morning", to reply "Is she? By what?" is a sort of poor joke.

In the same way it's something of a joke, conscious or unconscious, to puzzle a busy scientist who speaks of some things as having invisible currents and of others as not, by asking him "But are there any invisible currents?"

In the same way, in a country where it is the custom to say of watches which sing fairy songs and disappear on Midsummer Night that they are haunted by invisible leprechauns, it is something of a joke to say "*Are* there any invisible leprechauns? How do you know now that there is in this watch an invisible leprechaun?" Of course, in so far as the watch's behaviour, though leprechaun-like till now, is still in doubt, and in so far as there still lingers in the country a primitive idea about leprechauns so that the people of that country half expect to see one if they open a watch quickly, so far the interrogative air is appropriate and not a joke.

Likewise with "Has his soul fled?" While we still wonder whether a specialist might revive him or still half expect to see his rha fly slowly away at evening, the question is a question. But in these enlightened days no such thing is expected, so that when we know he'll never smile again then to ask "Is he dead?" becomes a joke.

"Ah", it may be said, "now here comes out a big difference. I see now why you say, 'To ask "Is there a current within?" "Is there a leprechaun within or a brownie?" is to ask which fairy tale shall we choose to remind us of the facts'. I see now how in the country you described it would be absurd to ask of a watch which sang fairy songs and disappeared on Midsummer Night, 'Is there a leprechaun within it?' and equally absurd to ask this question of a watch which did nothing extraordinary. But it's different with 'I know he won't speak or smile or give us any sign but still I can't help wondering whether he's lingering here or watching from another place'. This expresses not a joke but an old hope."

II.—WHAT IS DIALECTIC?

By Karl R. Popper.

There is no theory or opinion, however absurd or incredible, which has not been maintained by some one or other of our philosophers.
—Descartes.

I. Dialectic Explained.

The above motto can be generalised. It is not only valid within the field of philosophy. It appears to be applicable throughout the field of human thought and of human enterprise in general—throughout the whole field of science, technique and policy. And one can say, perhaps, that it would apply, if suitably reformulated, to a still wider field, namely, in considering the forms and appearances of life in general.

For if we want to explain why human thought tends to try out all sorts of conceivable solutions of any problem with which it is faced, then we can refer to a very general sort of regularity. The method by which a solution is approached is usually the same: it is the *method of trial and error*. Fundamentally it is the same method as that applied by living organisms, in the process of adaptation. It is clear that the success of this method depends very largely on the number and variety of the attempted trials: the more we try, the more likely it is that one of our attempts will be successful.

Thus, the method applied in the development of human thought in general, and of philosophy in particular, can well be described as a certain kind of trial and error method. Often, human beings seem inclined to react either in such a way that they emphatically assert some suggested theory and hold to it, trying it out for as long as they can (if it is an error, they may even perish with it), or they emphatically fight against such a theory, once they have discovered its weaknesses. This struggle of ideologies, which is, obviously, explicable in terms of the trial and error method, seems to be a characteristic feature wherever we find anything like a development of human thought. Only where a certain theory or system is dogmatically maintained throughout some longer period does it not occur. We

hardly ever find a continuous development, slow and steady, by continuous degrees of improvement.

If the method of trial and error is developed more and more consciously, then it begins to take on the characteristic features of *scientific method*. This method can briefly be described thus : Faced with a certain problem, the scientist offers tentatively some sort of solution—a theory. But this theory is at first only tentatively accepted by science ; and it is the most characteristic feature of scientific method that scientists will do everything they can in order to criticise and to test the theory in question. Criticising and testing go hand in hand : the theory is criticised from very many different standpoints in order to bring out those points which may be vulnerable. And the testing of the theory proceeds by its vulnerable sides to as severe an examination as possible. This again is the trial and error method. Theories are put forward tentatively and tried out. If the outcome of a test shows that the theory is erroneous, then it is eliminated : the trial and error method is essentially a *method of elimination*. Its success depends mainly on three conditions, namely, that sufficiently many and sufficiently different theories are offered, and that sufficiently severe tests are made. In this way we may secure, if we are lucky, the survival of the fittest theory by a process of elimination.

Assuming this description [1] of the development of human thought in general and of scientific thought in particular to be more or less correct, we are able to understand why such a development can be said to proceed on ' dialectic ' lines. Dialectic is a theory which maintains that something—for instance, human thought—develops in a way characterised by the so-called dialectic triad : thesis, anti-thesis, synthesis. First, some idea or theory or movement is given, which may be called " thesis ". Such a thesis will often produce opposition, because probably it will be, like most things in this world, of limited value—it will have its weak spots. This opposing idea or movement is called " anti-thesis " because it is directed against the first, the thesis. The struggle between the thesis and the anti-thesis goes on until some solution develops which will, in a certain sense, go beyond both thesis and anti-thesis by recognising the relative value of both, *i.e.*, by trying to preserve the merits and to avoid the limitations of both. This solution, which is the third step, is called " synthesis ". Once attained, the synthesis may in turn become the first step of a new dialectic triad, and it will, if the

[1] A more detailed discussion can be found in my book, *Logik der Forschung* (1935).

development does not stop with the particular synthesis reached. Probably the development will not stop, if the synthesis is not entirely satisfactory. For if it is not, it will arouse opposition, and can then be described as a new thesis which has produced a new antithesis : the dialectic triad will proceed at a higher level, and it will reach a third level when a second synthesis is attained.

So much for what is called " dialectic triad ". It can hardly be doubted that the dialectic triad describes certain developments fairly well, especially developments of ideas and theories, or of movements which are based on ideas or theories. Such a dialectic development can be *explained* by showing that it proceeds in conformity with the method of trial and error which we have discussed above. But it has to be admitted that it is not exactly the same as the trial and error development. Our description of the trial and error method only speaks about some idea and its criticism, or, using the terminology of dialecticians, about a thesis and its antithesis. But it makes no suggestions about the further development—it does not maintain that a struggle between a thesis and an antithesis will lead to a synthesis. Rather it would tend to suggest that the struggle between an idea and its criticism, or of a thesis and its antithesis will lead to an elimination of the thesis (or, perhaps, of the antithesis) if it is not satisfactory ; and to the establishment of another theory only if enough are at hand and are offered for trial.

Thus, the interpretation in terms of the trial and error method may be said to be slightly wider than that in terms of dialectic. It is not confined to a situation where only one thesis is offered to start with, and it can easily be applied to situations where from the very beginning different theses are offered independently of one another, and not only in such a way that the one is opposed by the other. But it has to be admitted that it happens very frequently, perhaps usually, that the development within a certain branch of human activity starts with one single idea only. If so, then the dialectic scheme may often be applicable because this thesis will be open to criticism and thus " produce ", as dialecticians usually say, its antithesis.

The dialectician's emphasis on the synthesis involves still another point where dialectic may differ slightly from the trial and error theory. For the trial and error theory as suggested above will be content to say that an unsatisfactory standpoint will be refuted or eliminated. The dialectician insists that there is more to be said than this. He emphasises that although the

theory under consideration may have been refuted, there will be most probably something in it worthy of being preserved, for otherwise it is not very likely that the theory would have been offered and taken seriously. And this valuable feature of the thesis will be made more and more clear by those who defend the thesis against the attacks of their opponents, the adherents of the antithesis. Thus a satisfactory solution of the struggle will be only a synthesis, *i.e.*, a theory in which the best points of both thesis and antithesis are preserved. It must be admitted that such a way of interpreting a certain development is sometimes very satisfactory, and that it is quite a valuable asset within the trial and error view-point.

Let us take the development of physics as an example. We can find very many instances of a theory which after its refutation remains "preserved" in the new theory which replaces it. To put it more precisely: the old formulæ become, from the standpoint of the new ones, approximations, that is, they appear to be very nearly correct, and they can be applied either where we do not demand a very high degree of exactness or, within a certain limited field of application, even as perfectly exact formulæ.

All this can be said in favour of the dialectic viewpoint. But we have to be very careful not to admit too much. We must be very careful, for instance, with a number of metaphors used by dialecticians and, unfortunately, often taken much too seriously. An example is the dialectical saying that the thesis "produces" its antithesis. Actually, it is only our critical attitude which produces the antithesis, and where such an attitude is lacking—which often enough is the case—no antithesis will be produced. Similarly, we have to be careful not to think that a struggle between a thesis and its antithesis will *always* "produce" a synthesis. There are many instances of very futile struggles in the history of human thought, struggles which ended in nothing. And even when a synthesis has been reached, usually it will be a rather crude description of the synthesis to say that it "preserves" the better parts of both, the thesis and the antithesis. Crude, at least, in the sense that such a description may be a possible way of looking at the situation but not an extremely enlightening one, because the synthesis will, in many cases, embody some idea or other which is entirely new and not capable of being reduced, without doing injustice, to foregoing stages of the development. In other words, the synthesis usually will be much more than a construction built merely of material supplied by thesis and antithesis.

Thus, the dialectic interpretation, although not inapplicable, will hardly ever *promote development* by attempting to construct the synthesis out of suggestions which can be gained from the thesis and the antithesis. This is a point which dialecticians are often prepared to stress themselves, but in practice they do not act accordingly, for they always hope that dialectic will enable them to predict the characteristic features of developments to come.

Another point where we have to be very careful is the loose way in which dialecticians speak about contradictions. For there is only one way of criticising a given theory : to show that either it is self-contradictory, or it is contradicted by some other accepted statement, either by other theories or by statements about facts—a case which we usually describe by saying that the theory in question is contradicted by the facts. Thus, any sort of criticism of a theory will *contradict* this theory (and can therefore be called an antithesis). Now, the dialecticians sometimes emphasise that such a contradiction between a thesis and an antithesis is extremely productive, is the very promoter of progress, and that we are therefore quite wrong in assuming, and that logic is quite wrong in teaching, that contradictions are something always to be avoided. They even go so far as to say that a contradiction is something which, quite naturally, occurs everywhere in the world.

I shall discuss this assertion of the dialecticians later more extensively. For the moment, I should like only to emphasise one point : criticism is often productive, and if we want to, we may call certain contradictions " productive ". But they are productive *only because we try to avoid them*, to improve on them, to supersede them. If we did not try to avoid contradictions, then we should have no reason whatsoever to describe the relation between a thesis and its antithesis as something which has to be superseded—which has to be settled by establishing a synthesis. Then we should have no reason whatsoever to say that there is something like a struggle between thesis and antithesis, or like a tension which is a productive force in the development.

The only " force " which promotes the dialectic development is, therefore, *our reluctance* to accept, and to put up with, the contradiction between the thesis and the antithesis. It is not a mysterious force inside these two ideas, not a mysterious tension between them which promotes development—it is purely our decision, our resolution, not to agree to contradictions, which induces us to look out for a new standpoint enabling us to avoid them.

And this resolution is entirely justified. For it can easily be shown that if one were to accept contradictions then one would have to give up any kind of scientific activity: it would mean a complete break-down of science. This can be shown by proving that if two contradictory sentences are admitted, any sentence whatsoever must be admitted.[1] In order to make this point entirely clear I must first prepare the way by expounding two logical rules of deduction, which may demand a little patience of the reader.

All sciences try to promote theories, that is, deductive systems of statements. Such deductive systems consist of certain assumptions and their logical consequences, *i.e.*, statements which are deduced from the assumptions. These deduced statements are often called conclusions. Now the deduction of conclusions proceeds according to certain rules, the so-called rules of deduction. Some of these rules are rather trivial, for instance, the two rules of which I shall make use (*cf.* (1) and (2) below).

In order to explain these two rules, I shall make use of the symbols "p", "q" and "r", representing any statements whatsoever. For instance, the symbol "p" may be taken to represent the statement "Socrates is wise". By "not-p", I symbolise the negation of "p". Thus, if we choose to substitute for "p" the statement "Socrates is wise", then we have to substitute for "not-p" the statement "Socrates is not wise" or "It is not the case that Socrates is wise".

Furthermore, I shall make use of the symbol "or". In ordinary language, this word is used ambiguously. If I say, "It will rain to-morrow *or* it will not (but not both)," then I am using the word "or" in order to express that the two possibilities are exclusive. But there are other meanings of "or". For instance, if somebody says: "I shall have a sandwich *or* a piece of cake (possibly both)." Here the word "or" is used in a *non-exclusive* sense—it expresses that *at least* one of the two possibilities will come true, possibly both of them. Similarly, in what follows, the symbol "or" shall be used in a strictly non-exclusive sense; this sense can be precisely determined if we agree to use the word "or" in accordance with the following convention:

A compound statement of the form "p or q" (where for the symbol "p" and for the symbol "q" any sentence may be

[1] This fact is not always realised (and shall therefore here be fully dealt with); *cf.* H. Jeffreys, "The Nature of Mathematics" (*Philosophy of Science*, V, 449): "Whether a contradiction entails any proposition is doubtful."

substituted) shall be said to be true if, and only if, at least one of its two constituent sentences, represented by " p " and by " q " respectively, is true.

Accordingly the statement " p or q ", as used here, does not maintain more than that *at least* one of its constituents is true (and possibly both).

Making use of the symbol " or " in this way, it follows at once that from any given single premiss—which may be symbolised by " p "—any composite conclusion of the form " p or q " can be deduced, where " p " symbolises the given premiss and " q " any sentence whatsoever. Thus, from the premiss " Socrates is wise " the conclusion " Socrates is wise or Peter is a king " can be deduced, and equally well, " Socrates is wise or Peter is not a king ". This result which may appear rather strange to those who are not used to such an analysis becomes obvious if we remember our convention as to the use of " or ". For, according to this convention, the sentence " Socrates is wise or Peter is a king " must be true if the sentence " Socrates is wise " is true. In other words: whoever admits " p ", is bound to admit " p or q ", for " p or q " must be true if " p " is true.

Thus we come to the first rule of deduction of which I must make use, namely,

(1) From any given premiss, symbolised, for instance, by " p ", we are entitled to deduce a conclusion of the form " p or q " (where " p " represents the same sentence as was chosen as the premiss, and where " q " represents any sentence whatsoever).

The second rule of deduction of which I have to make use is even more obvious:

(2) From two given premisses of the form:

(a) " p or q "
(b) " not-p "

we are entitled to deduce the conclusion " q ".

For example: Given the two premisses, " Socrates is wise or Peter is a king (perhaps both) and " Socrates is not wise ", we are entitled to deduce the conclusion " Peter is a king "; in other words, this statement is a logical consequence of the two foregoing ones: Whoever admits the two premisses is bound to admit the conclusion.

That this must be so follows, again, from the meaning we have attached to the symbol " or ". As the second premiss " not-p " informs us that " p " is not true, it follows from the meaning of " or " that, if the premisses are true, " q " must be true.

It may be worth noting that this analysis must not be attacked by saying that it misrepresents the real meaning of "or". It is true that this meaning is, by our analysis, over-simplified, that it does not correspond any longer to the ordinary meaning of "or". But all that we are going to base on the above analysis —namely, the proof that the introduction of contradictory sentences must lead to a complete break-down of science—is completely independent of the *ordinary* use of "or". We keep consistently to *our* use of it. And we could easily restate the whole argument without mentioning the word "or" at all, by introducing some artificial symbol like "*i*" or "*V*" instead of it, and our argument would remain valid.

It is a fact that everyone, and dialecticians are no exception, makes (perhaps unconsciously) use of the aforesaid two rules of deduction. But, with the help of these two rules, we can proceed to the proof which I have undertaken to give. For it can easily be shown that these rules permit us to deduce from a pair of contradictory sentences, for instance, from the two sentences, "The sun is shining" and "The sun is not shining", *any sentence whatsoever*.

Let us take these two premisses:

(*a*) "The sun is shining"
(*b*) "The sun is not shining".

We can deduce with the help of rule (1) from the first of these premisses, the following sentence: "The sun is shining or Cæsar was a traitor". But from this sentence, together with the second premiss (*b*), we can deduce, following rule (2), that Cæsar was a traitor. And by the same method we can deduce any other sentence. This is extremely important, for if we can deduce any sentence whatsoever, then, clearly, we can always deduce any negation of any sentence whatsoever: It is clear that instead of the sentence "Cæsar was a traitor" we can, if we wish, deduce "Cæsar was not a traitor". In other words, from two contradictory premisses, we can logically deduce anything, and its negation as well. *We therefore convey with such a contradictory theory—nothing.* A theory which involves a contradiction is entirely useless, because it does not convey any sort of information.

From this, we see the real significance of the so-called "law of contradiction". This logical rule, which forbids contradictions, thereby inducing us never to accept any contradiction, secures the possibility of conveying something with the help of a deductive system. Once a contradiction were admitted, all science would collapse.

One sees here the extreme danger of loose and metaphorical ways of expression. The looseness of the dialectician's saying that contradictions are not avoidable and that avoidance is not even desirable because of their fertility, is dangerously misleading. It is misleading because what may be called the fertility of the contradictions is only an outcome of the fact that we don't want to put up with them, an attitude which accords with the law of contradiction. And it is dangerous, because to say that the contradictions need not be avoided, or perhaps even that they cannot be avoided, must lead to the above-mentioned scientific break-down.

This may emphasise that it is a necessity and even a duty for everyone who wants to promote truth and enlightment, to train himself in the art of expressing things clearly and unambiguously—even if this means giving up certain niceties of metaphor and clever double meanings.

Therefore, one had better avoid certain formulations. For instance, instead of the terminology we have used in speaking of thesis, antithesis and synthesis, dialecticians often describe the dialectic triad by using the term "negation" instead of "antithesis" and "negation of the negation" instead of "synthesis". Such terminology would do no harm if these terms "negation" and "negation of the negation" had not clear and definite logical meanings, different from the dialectic usage. Actually, the misuse of these terms has contributed considerably to the mixing-up of logic and dialectic which so often occurs in the discussions of the dialecticians: frequently they consider dialectic to be a part—the better part—of logic, or something like a reformed, modernised logic. The deeper reasons for such an attitude will be discussed later. At present, I only want to mention that our analysis does not lead us to assume that dialectic has any sort of similarity to logic. For logic can be described—roughly, but well enough for our present purposes—as a theory of deduction. We have no reason to believe that dialectic has anything to do with deduction.

To sum up: What dialectic is—dialectic in the sense in which we can attach a clear meaning to the dialectic triad—can be described in the following manner.

Dialectic, or more precisely, the dialectic triad, maintains that certain developments, or certain historical processes, occur in a certain typical way. It is, therefore, an empirical descriptive theory, comparable, for instance, with a theory which maintains that most living organisms increase their size during some stage of their development, then remain constant, and lastly decrease

until they die ; or with the theory which maintains that opinions are usually held first in a dogmatic attitude, then in a somewhat sceptical attitude, and only afterwards, in a third stage, in a scientific, *i.e.*, critical, attitude. Like such theories, dialectic is not applicable without exceptions—as long as we are careful not to force the dialectic interpretation. Like those theories, dialectic is rather vague. And like those theories, dialectic has nothing particular to do with logic.

The vagueness of dialectic is another of its dangers. It makes it only too easy to force a dialectic interpretation on all sorts of developments and even on quite different things. We find, for instance, a dialectic interpretation which identifies a seed corn with a thesis—the plant which develops from this seed corn with the antithesis—and all the seeds which develop from this plant with the synthesis. That such an application expands the already too vague meaning of the dialectic triad in a way which dangerously increases its vagueness is obvious : it leads to a stage where by describing a development as a dialectic development we do not convey any more than by saying that it is a development in stages—which is not very much. But to interpret the above-mentioned development by saying that germination of the plant is the negation of the seed because the seed ceases to exist when the plant begins to grow, and that the production of a lot of new seeds by the plant is the negation of the negation—a new start on a higher level—is obviously a mere playing with words. (Is this the reason why Engels said of this example that any child can understand it ?)

A theory like logic can be called " fundamental ", thereby indicating that, since it is the theory of all sorts of inferences, it is used all the time by all the sciences. We can say that dialectic in the sense in which we found that we could make a sensible application of it is not a fundamental but merely a descriptive theory. It is therefore about as inappropriate to take dialectic as being part and parcel of logic, or else as being opposed to logic, as it would be to take, say, the theory of evolution. Only the loose, metaphorical and ambiguous way of speaking which we have criticised above could make it appear that dialectic can be *both* a theory describing certain typical developments *and* a fundamental theory such as logic.

From all this it is, I think, clear, that one should be very careful in using the term " dialectic ". It would be best, perhaps, not to use it at all—we can always explain such developments in the clearer terminology of a trial and error development. Exceptions should be made only where no misunderstanding

is possible, and where a certain development really proceeds in a very striking way along the lines of a triad.

II. Hegelian Dialectic.

So far I have tried to outline the idea of dialectic in a way which, I hope, makes it understandable, and it was my aim not to be unjust regarding its merits. In this outline dialectic is presented as a way of describing developments; as one way among others, not of fundamental importance, but sometimes quite a suitable one. As opposed to this, dialectic usually has been put forward, especially by Hegel and his school, in an exaggerated and dangerously misleading form.

In order to make Hegel's dialectic understandable it may be useful to refer briefly to a chapter of the history of philosophy—in my opinion not a very pleasant one.

A major issue in the history of modern philosophy is the struggle between the (mainly continental) Cartesianism or *rationalism* on the one side, and the (mainly British) *empiricism* on the other side. Descartes' sentence which I have used as a motto for this paper, is meant by its author, the founder of the rationalistic school, not in the way in which I have made use of it to start with: it is not intended to indicate that the human mind has to try everything in order to arrive at something—*i.e.*, at some useful solution—but rather as a hostile criticism of those who dared to try out all these absurdities. What Descartes had in mind, the main idea behind his sentence, is that the real philosopher should carefully avoid all those absurd and foolish ideas. In order to find truth, he has only to accept the one idea which recommends itself by its reasonableness, which appeals to reason by its lucidity, by its clearness and distinctness, in short, which is self-evident. The Cartesian idea is that we can construct the whole body of science without any reference to experience, just by making use of our reason, for every reasonable proposition (recommending itself by its lucidity) must be a true description of the facts. This, in short, is the theory which the history of philosophy called "rationalism". It can be summed up (using a formulation of a much later, namely the Hegelian, period) in the words: "That which is reasonable must be real!"

As opposed to this theory, empiricism maintains that only experience enables us to decide upon the truth or falsity of a scientific theory. Pure reasoning alone, according to empiricism, can never help us to find the truth; rather, we have to make use of observations and experiments. It can safely be said that some or other form of empiricism, although perhaps a modest

and modified form, is the only interpretation of scientific method which can be taken seriously in our day. The struggle between the earlier rationalists and empiricists was thoroughly discussed by Kant, who tried to offer some synthesis—a compromise, or rather, a modified form of empiricism. His main interest was to reject pure rationalism. In his *Critique of Pure Reason*, he maintained that the scope of our knowledge is limited to the field of possible experiences and that pure reasoning beyond this field—the attempt to build up a metaphysical system out of pure reason—has no justification whatsoever. This criticism of pure reason was felt as a terrible blow to the hopes of nearly all continental philosophers, and in Germany especially, far from being content with Kant's refutation of metaphysics, philosophers busied themselves with building up new metaphysical systems based on pure speculation, thereby trying to use certain features of Kant's system in order to outdo him. The school thus developed, usually called the school of the German idealists, culminated in Hegel.

There are two points in Hegel's philosophy which we have to discuss—his idealism and his dialectic. In both points Hegel followed Kant, trying to go beyond him. In order to understand Hegel we must therefore outline the roots of his theory in the work of Kant.

Kant started from the fact that science exists. He wanted to *explain* this fact; that is, he wanted to answer the question: "How is science possible?" In other words: How are human minds able to have knowledge of the world? or in short: *How can our mind grasp the world?* (We could call this question the epistemological problem.)

His reasoning was somewhat as follows: The mind can grasp the world or, rather, the world as it appears to us, because this world is not utterly different from our mind—*because it is mind-like.* And it is so, because in the process of obtaining knowledge, of grasping the world, our mind is, so to speak, actively digesting all that material which enters it by our senses. It is forming, moulding this material; it impresses on it its own intrinsic forms—the forms of our thought. What we call "nature"—the world in which we live, the world as it appears to us—is already a world digested, is a world formed by our mind. And being thus assimilated by the mind, it is mind-like.

The answer, "Our mind can grasp the world because the world is mind-like" is a typical *idealistic* argument; for what idealism asserts is just that the world is somewhat of the character of our mind.

I do not intend to argue the pros and cons of this Kantian epistemology and I do not intend to discuss it in detail. But I want to stress that it certainly is not entirely idealistic. It is a mixture, or synthesis, as Kant himself points out, of some sort of realism and some sort of idealism—its realistic element being that the world as it appears to us is some sort of *material* formed by our mind, whilst its idealistic element is, that it is some sort of material *formed by our mind*.

So much for Kant's rather abstract but certainly ingenious epistemology. Before I proceed to Hegel, I must warn those readers (I like them most) who are no philosophers and are used to relying on their common sense, to bear in mind the sentence which I chose as a motto for this paper; for what they will hear now will probably appear to them, in my opinion quite rightly, to be absurd.

As I have said, Hegel in his idealism went beyond Kant. Hegel, too, was concerned with the question: "How can our mind grasp the world?" But his theory—with the other idealists he also answered: "Because the world is mind-like" —was more radical than Kant's. He did not say, like Kant: "Because our mind *forms* the world", he said: "Because our mind *is* the world"; or in another formulation: "Because the reasonable *is* the real—because reality and reason are *identical*."

This is Hegel's so-called "philosophy of identity of reason and reality", or, in short, his "philosophy of identity". It is hardly worth noting that between Kant's: "Because our mind *forms* the world", and Hegel's philosophy of identity: "Because our mind *is* the world" there was, historically, a bridge—namely Fichte's answer: "Because our mind *creates* the world."

Hegel's philosophy of identity "That which is reasonable is real, and that which is real is reasonable; thus, reason and reality are identical" was doubtless an attempt to re-establish rationalism on a new basis. It permitted the philosopher to construct a theory of the world out of pure reasoning and to maintain that this must be a true theory of the real world. Thus it allowed exactly that which Kant had said to be impossible. Hegel, therefore, was bound to refute Kant's arguments against metaphysics. He did this with the help of his dialectic.

To understand his dialectic, we have thus again to go back to Kant. To avoid details, I shall not discuss the triadic construction of Kant's table of categories, although it doubtless inspired Hegel. But I have to refer to Kant's method in rejecting rationalism. I mentioned above that Kant maintained that

the scope of our knowledge is limited to the field of possible experience and that pure reasoning beyond this field is not justified. To show this, he proceeded (in a section of the "Critique" which he headed "Transcendental Dialectic") in the following way. If we try, he showed, to construct a theoretical system out of pure reason, for instance, if we try to argue that the world in which we live is infinite (which obviously goes beyond possible experience), we can do so—but we shall find that we can always argue with the help of analogous arguments in an opposite direction as well. In other words, given such a metaphysical thesis, we always could construct and defend an exactly opposite antithesis; and for any argument which speaks in favour of the thesis, we can easily construct its opposite argument in favour of the antithesis. And both arguments will carry with them a very similar force of conviction—both arguments will appear to be equally, or nearly equally, reasonable. Thus, Kant said, reason is bound to argue against itself and to contradict itself, if used to go beyond possible experience.

If I were allowed to give some sort of modernised reconstruction, or re-interpretation, of Kant, deviating from Kant's own view of what he had done, I should say that Kant showed that the metaphysical principle of reasonableness or self-evidence does not lead unambiguously to one and only one result, to one and only one theory. Rather, it is always possible to argue, with similar apparent reasonableness, in favour of a number of different theories, and even of opposite theories. Thus, if we do not get help from experience, if we cannot make experiments or observations which tell us, at least, to eliminate certain theories—namely those which, although they may seem quite reasonable, are contrary to the observed facts—then we have no hope of ever settling the claims of different competing theories.

But how did Hegel overcome the obstacle of Kant's refutation of rationalism? Very easily: he simply said that contradictions do not matter. They just have to occur in the development of thought and reason. They are only showing the insufficiency of a theory which does not take account of the fact that thought, reason, and with it (according to the philosophy of identity) reality, is not something fixed once and for ever, but that it is developing—that we live in a world of evolution. Kant, so says Hegel, refuted metaphysics, but not rationalism. For what Hegel calls "metaphysics", as opposed to "dialectic", is only such a rationalistic system as does not take account of evolution, motion, development, and thus tries to conceive of reality as something stable, unmoved and free of contradictions. Hegel,

with his philosophy of identity, infers that, as reason develops, the world must develop, and as the development of thought or reason is a dialectic one, the world must also develop in dialectic triads.

Thus we find the following three elements in Hegel's dialectic :

(*a*) An attempt to supersede Kant's refutation of what Kant called rationalism or metaphysics. This refutation is recognised by Hegel to hold only for systems which are metaphysical in his sense, but not for dialectical rationalism, that is, a kind of rationalism which takes account of the development of reason and is therefore not afraid of contradictions. I wish to remark that in superseding Kant's criticism in this way, Hegel embarks on an extremely dangerous venture which must lead to disaster, for he argues in somewhat the following fashion : " Kant refuted rationalism by saying that it must lead to contradictions with no possibility of avoiding them. I admit that. But it is clear that this argument draws its force from the law of contradiction : it refutes only such systems as want to be free from contradictions. It is not dangerous for a system like mine which is prepared to put up with contradictions—that is, for a dialectic system." It is clear that this argument establishes a dogmatism of an extremely dangerous kind—a dogmatism which has no need to be afraid of any sort of attack. For any attack, any criticism of any theory whatsoever, must be based on the method of pointing out some sort of contradictions, either within the theory itself or between the theory and some facts—as I have already mentioned above. Hegel's method of superseding Kant, therefore, is effective, but unfortunately too effective. It makes his system secure against *any* sort of criticism or attack and thus is dogmatic in a very peculiar sense, so that I should like to call it a " re-inforced dogmatism ". (It may be remarked that we find such re-inforced dogmatism embodied in various dogmatic systems.)

(*b*) The description of the development of reason in terms of dialectic is an element in Hegel's philosophy which carried with it a considerable amount of plausibility. This becomes clear if we remember that Hegel uses the word " reason " not only in the subjective sense denoting a certain mental capacity, but in the objective sense as well, namely, denoting all sorts of theories, thoughts, ideas and so on. Hegel, who holds that philosophy is the highest expression of reasoning, has in mind mainly the development of philosophical thought when he speaks of the development of reasoning. And indeed there is hardly a field to which the dialectic triad can be more successfully applied than the development of philosophical theories, and it is

therefore not surprising that Hegel's most successful attempt at applying his dialectic method was his *History of Philosophy*.

In order to understand the danger connected with such a success, we have to remember that in Hegel's time—and even much later—*logic* was usually described and defined as the *theory of reasoning* or the theory of thinking, and accordingly the fundamental laws of logic usually were called the " laws of thought ". Therefore, it is not quite incomprehensible that Hegel, believing that dialectic is the true description of our actual procedure when reasoning or thinking, held that he must alter logic so as to make dialectic an important, if not the most important, part of logical theory. This necessitated the discarding of the so-called " law of contradiction " which clearly was a grave obstacle to the embodiment of dialectic. Here we have the origin of the view that dialectic is " fundamental " in the sense that it can compete with logic, that it is an improvement upon logic. We have already criticised such a view of dialectic, and I only want to remark that any sort of logical reasoning, whether before or after Hegel, and whether in science or mathematics or any truly scientific philosophy, is always based on the law of contradiction. Hegel writes (*Logic*, Section 81, (1)): " It is of the highest importance to ascertain and understand rightly the nature of Dialectic. Wherever there is movement, wherever there is life, wherever anything is carried into effect in the actual world, there Dialectic is at work. It is also the soul of all knowledge which is truly scientific."

But if by dialectic reasoning Hegel means a reasoning which discards the law of contradiction, then he certainly would not be able to give any instance of such sort of reasoning in science. It is not scientific reasoning itself, not scientific arguing, which is *based* on dialectic, but it is only the *development* of scientific theories which can, with a certain amount of success, be *described* in terms of the dialectic method. As we have seen already, this fact cannot justify the acceptance of dialectic as something fundamental, because it can be comparatively easily explained without leaving the realm of ordinary logic if we remember the working of the trial and error method.

The main danger of such a mix-up of dialectic and logic is, again, that it offers help for arguing dogmatically. For we find only too often that dialecticians, when in logical difficulties, answer their opponents, as a last way out, that their criticism is mistaken, being based on logic of the ordinary type instead of on dialectic ; if they would only use dialectic, then they would see that the contradictions which they found in some arguments

of the dialecticians are quite legitimate—namely from the standpoint of dialectic.

(*c*) A third element of Hegelian dialectic is based on his philosophy of identity. If reason and reality are identical and reason develops dialectically (as so well exemplified by the development of philosophical thought) then reality must develop dialectically too: the world must be ruled by the laws of dialectical logic. (This standpoint has been called " panlogism ".) Thus, we must find in this world the same contradictions as are permitted by dialectic logic. It is this very fact, namely, that the world is full of contradictions which shows us from another side that the law of contradiction has to be discarded. For this law says that no self-contradictory proposition, or no pair of contradictory propositions, can be *true*, that is, can correspond to the facts. In other words, the law implies that a contradiction can never occur within the facts, that facts can never contradict. But if on the basis of the philosophy of identity of reason and reality, the theory is asserted that facts can be contradictory and can contradict each other, then it is clear that the law of contradiction has to be abandoned.

But apart from what appears to me to be the utter absurdity of the philosophy of identity (about which I shall say a few words later): if we look a little closer into these so-called contradictory facts, then we find that all the various examples proffered by dialecticians just illustrate that the world in which we live shows, sometimes, a certain structure which could perhaps be described with the help of the word " polarity ". An instance of that structure would be the existence of positive and negative electricity. It is only a metaphorical, a very loose, way of speaking, if we, for instance, say that positive and negative electricity are contradictory to each other. An example of a contradiction would be the following two sentences: " This body here was, on the 1st of November, 1938, between 9 and 10 a.m., charged positively," and an analogous sentence about the same body, saying that it was at the same time not positively charged.

This would be a contradiction between two sentences and the corresponding contradictory fact would be the fact that a body would be at the same time both positively and not positively charged, that is, at the same time attract and not attract certain negatively charged bodies. But we need not say that such contradictory facts do not exist. (A deeper analysis might show that the non-existence of such facts is not a law of the type of the laws of physics, but is based on logic, that is, on the rules governing the use of scientific language.)

These three points, namely, (*a*) the dialectic opposition against Kant's anti-Rationalism, and consequently, the re-establishment of rationalism supported by a reinforced dogmatism, (*b*) the incorporation of dialectic into logic, on the ground of the ambiguity of expressions like " reason ", " laws of thought ", and so on, and (*c*) the application of dialectic to " the whole world ", based on Hegel's panlogism and his philosophy of identity,—these three points seem to me to be the main elements within Hegelian dialectic. Before I proceed to outline dialectic after Hegel, I should like to express my personal opinion about Hegel's philosophy, and especially about his philosophy of identity. I think it represents the worst of all those absurd and incredible philosophic theories to which Descartes refers. It is not only that the philosophy of identity is offered without any sort of serious justification ; even the problem to answer which it has been invented—the question " How can our mind grasp the world ? " —seems to me not a clearly formulated problem at all. And the idealistic answer, which has been varied by different idealistic philosophers but remains fundamentally the same, namely " Because the world is mind-like ", has only the appearance of an answer. But we shall see clearly that it has not the slightest justification, if we only consider some analogous argument, like : " How can this mirror reflect my face ? "—" Because it is face-like." Although this sort of arguing is obviously utterly unsound, it has been offered again and again. We find it for instance, in our time, formulated by Jeans [1] in somewhat the following fashion : " How can mathematics grasp the world ? "—" Because the world is mathematic-like ". He argues, in that way, that reality is of the very nature of mathematics—that the world is a mathematical thought (and therefore ideal). This argument obviously is by no means sounder than the following : " How can language describe the world ? "—" Because the World is language-like—it is linguistic," and more particularly : " How can the English language describe the world ? "—" Because it is intrinsically British ". That this latter argument really is analogous to the one offered by Jeans, is easily seen if we recognise that the mathematical description of the world is just a certain way of describing the world and nothing else, and that mathematics is a means of description—that is : a certain language.

One can show this most easily perhaps with the help of a trivial example. There are primitive languages which do not employ numbers but try to express numerical ideas with the

[1] Other and much more elaborate critical remarks on Jeans' Philosophy can be found in L. S. Stebbing's excellent book, *Philosophy and the Physicist*.

help of expressions for one, two, and many. It is clear that such a language is unable to describe many of those more complicated relationships between certain groups of objects, which can easily be described with the help of the numerical expressions " three ", " four ", " five ", and so on. In other words, mathematical symbols are introduced in a language in order to describe certain more complicated relationships which could not be described otherwise ; a language which contains mathematics is, simply, very much richer than a language which has no such symbols. All that we can infer about the nature of the world from the fact that we have to use mathematical language if we want to describe it, is that this world has a certain degree of complexity or that there are certain relationships in this world which cannot be described with too primitive means.

It seems that there is still another point in Jeans' uneasiness regarding the fact that our world happens to suit mathematical formulæ originally invented by pure mathematicians who do not intend at all to apply their formulæ to this world. Jeans was, so it seems at least, originally an inductivist, that is, he thought that theories are obtained from experience by some more or less simple procedure. From such a standpoint it obviously is astonishing to find that a theory which has been formulated by pure mathematicians, in a purely speculative manner, afterwards proves to be applicable to the physical world. But for those who are not inductivists it is not astonishing at all. They know that it happens quite often that a theory put forward originally as a pure speculation, as a mere possibility, later proves to have its empirical applications. They know that often it is this speculative anticipation which prepares the way for the empirical theories. In this way, the so-called problem of induction has a bearing on the problem of idealism with which we are concerned here. But I must leave it with these brief allusions.

III. Dialectic After Hegel.

Hegel's philosophy of identity of reason and reality sometimes is characterised as (absolute) *idealism*, because it states that reality is mind-like or of the character of reason. But it is clear that such a philosophy of identity can easily be converted into some sort of *materialism*. It then could argue that reality is, in fact, of the material or physical character which the ordinary man thinks it to be ; and by saying that it is identical with reason, or mind, one would imply that the mind is also a material or physical phenomenon—or, in some less radical form, that if

mind should be somewhat different, then the difference cannot be of great importance.

Such a materialism can be considered to be a renaissance of certain features of Cartesianism, connected with dialectic. But in discarding its original idealistic basis, dialectic loses everything which made it plausible and understandable; we have to remember that the best arguments in favour of dialectic lay in its applicability to the development of thoughts, especially of philosophical thoughts. Now we are faced blankly with the statement that physical reality develops dialectically—an extremely dogmatic assertion with such little scientific backing that materialistic dialecticians are forced to make a very extensive use of the above-described dangerous method of discarding criticism by denouncing it as being non-dialectical. Dialectical materialism thus is in agreement with the points (*a*) and (*b*) discussed above, but it alters point (*c*) considerably, although I think not advantageously for its dialectic features. In expressing this opinion, I want to stress that although I should not describe myself as a materialist, my criticism is not directed against materialism, which I personally should probably prefer to idealism if I were forced to choose (which, fortunately, is not the case). It is only the combination of dialectic and materialism that appears to me to be even worse than dialectic idealism.

These remarks apply particularly to the so-called "Dialectical Materialism" as developed by Marx. The materialistic element in this theory could be comparatively easily reformulated in such a way that no serious objections could be made against it. As far as I can see the main point is this: That there is no reason to assume that, whilst the natural sciences can proceed on the basis of the common man's realistic outlook, the social sciences need an idealistic background like the one offered by Hegelianism. Such an assumption was often made in Marx's time, owing to the fact that Hegel with his idealistic theory of the State appeared to influence strongly, and even to further, the social sciences, whilst the futility of his views within the field of the natural sciences was—at least for natural scientists—only too obvious. I think that it is a fair interpretation of the ideas of Marx and Engels, to say that one of their chief interests in emphasising materialism is to dismiss any such theory as, referring to the rational or spiritual nature of man, maintains that sociology has to be based on an idealistic or spiritualistic basis, or on the analysis of reason. As opposed to this they stressed that the material side of human nature—and more particularly the need for food and other material goods—is of basic importance for sociology.

This view, doubtless, was sound ; and I hold Marx's contributions on this point to be of real significance and of lasting influence. Everyone learned from Marx that the development even of ideas cannot be fully understood if the history of ideas is treated purely as such (although such a treatment has its merits) apart from the conditions of their origin and of the situation of their originators, of which conditions the economic aspect is of the highest significance. Nevertheless I personally think that Marx's economism—his emphasis on the economic background as being the ultimate basis of any sort of development—is exaggerated. I think that social experience shows that under certain circumstances the influence of ideas supported by propaganda can outweigh and supersede economic forces. Besides, granted that it is not possible fully to understand mental developments without understanding their economic background : is it possible to understand the economic development without understanding the development, for instance, of scientific, or religious, ideas ?

For our present purpose it is not so important to analyse Marx's materialism and economism, but rather to see what has become of dialectic within his system. Two points seem to me of importance. One is Marx's emphasis on historical method in sociology, a tendency which I may call " historicism ". The other is the anti-dogmatic tendency of Marx's dialectic.

In regard to the first point we have to remember that Hegel was one of the inventors of the historical method—of the school of thinkers who believed that in describing a development historically one has causally explained it. This school believed that one could explain, for instance, certain social institutions by showing how mankind has slowly developed them. Now it is more or less recognised that the historical method has been in general very much over-rated. It is only in the field of the social sciences that historicism still has considerable influence. I have tried to criticise this method elsewhere (in a paper " The Poverty of Historicism " to be published soon). In the present paper I merely want to stress the fact that Marx's sociology adopted from Hegel not only the view that the method of sociology has to be the historical and that sociology has to become a theory of social development, but also the view that this development has to be explained in dialectical terms. To Hegel, history was the history of ideas. Marx dropped idealism but retained Hegel's standpoint that the dynamic forces of historical developments are the dialectical " contradictions ", " negations ", and " negations of negations ". In this respect Marx and Engels followed Hegel very closely indeed, as may be shown by the following quotations. Hegel

(in his *Encyclopædia*, Part, I, ch. vi, § 81) described Dialectic as "the universal and irresistible power before which nothing can stay, however secure and stable it may deem itself." Similarly, Engels writes (Anti-Duehring, Part I, "Dialectics: Negation of the Negation"): "What therefore is the negation of the negation? An extremely general . . . law of development of Nature, history and thought; a law which . . . holds good in the animal and plant kingdoms, in geology, in mathematics, in history, and in philosophy. . . ."

In Marx's view, it is the main task of sociological science to show how these dialectic forces are working in the field of history, and thus to prophesy historical development; or, as he says in the preface to *Capital:* "It is the ultimate aim of this work to lay bare the economic law of motion of modern society." And this dialectic law of motion, the negation of the negation, furnishes the basis of Marx's prophecy of the impending end of capitalism (*Capital*, I, ch. 24, § 7): "The capitalist mode of production . . . is the first negation. . . . But capitalism begets, with the inexorability of a law of Nature, its own negation. It is the negation of the negation."

Certainly, prophecy as such need not be unscientific, as predictions of eclipses and other astronomical events show. But Hegelian dialectic, or its materialistic version, cannot be accepted as a sound basis for scientific forecasts. ("But Marx's predictions have all come true", Marxists usually answer. They have not. To quote one example out of many: In *Capital*, immediately after the last quotation, Marx prophesied that the transition from capitalism to socialism must be, naturally, a process *incomparably less* "protracted, violent, and difficult" than the industrial revolution, and in a footnote he amplified this forecast by referring to the "irresolute and non-resisting bourgeoisie"). Thus, if forecasts based on dialectic are proffered, some will come true and some will not. In the latter case, obviously, a situation will arise which has not been foreseen. But dialectic is vague and elastic enough to interpret and to explain this unforeseen situation just as well as it explained and foretold the other situation which happened not to come true: Any development whatsoever will fit the dialectic scheme; *the dialectician need never be afraid of any refutation by forthcoming experiences.*[1] As mentioned before, it is not only the dialectical approach, but rather it is the very idea of approaching the problems of sociology

[1] In my book, *Logik der Forschung*, I have tried to show that the more a theory conveys, and the greater its scientific content is, the more it risks being refuted by forthcoming experiences; thus *dialectic is unscientific.*

historically, the idea that the large-scale historical forecast is the aim of scientific sociology, which is mistaken. But this does not concern us here.

Apart from the rôle dialectic plays in Marx's historical method, Marx's anti-dogmatic tendencies should be discussed. Marx and Engels strongly emphasised that science should not be interpreted as a body of finally and well-established knowledge, or of " eternal truth ", but rather as something developing, progressive. The scientist is not the man who knows a lot but rather the man who is determined not to give up searching for truth. Scientific systems develop ; and they develop, according to Marx, dialectically.

There is not very much to say against this point—although personally I think that the dialectical description of scientific development is, unless it is forced, not always applicable and that it is better to describe scientific development in a less ambiguous way, for instance, in terms of the trial and error theory. But I am prepared to admit that this criticism is not of great importance. It is, however, of real moment that Marx's progressive and anti-dogmatic view of science has never been applied by orthodox Marxists within the field of their own activities. Progressive, anti-dogmatic science is critical—criticism is its very life. But criticism of Marxism, of dialectic materialism, has never been tolerated by Marxists.

Hegel thought that while philosophy develops, his own system has to remain the latest and highest stage of this development and cannot be superseded. The Marxists adopted the same attitude towards the Marxian system. Hence, Marx's anti-dogmatic attitude exists only in the theory but not in the practice of orthodox Marxism, and dialectic is used by Marxists, following the example of Engels' Anti-Duehring, mainly to defend the Marxist system against criticism, and not to criticise it or to develop it. As a rule, critics are denounced as not understanding dialectic, or as unable to understand proletarian science, or as traitors. Thanks to dialectic the anti-dogmatic tendencies have disappeared, and Marxism has established itself as a dogmatism which is elastic enough, by using its dialectic method, to evade any further attack—in short, as a reinforced dogmatism.

It is clear that nothing can damage scientific development more than dogmatism. There can be no scientific development without free competition of thought—this is the essence of the anti-dogmatic attitude once so strongly supported by Marx and Engels. *And there cannot be free competition of scientific thought without freedom of thought.*

Thus, dialectic has played a very unfortunate rôle not only in the development of philosophy, but also in the development of political theory. A full understanding of this unfortunate rôle will be easier if we try to see how Marx originally came to develop such a theory. We have to consider the whole situation: Marx, a young man who was progressive, evolutionary and even revolutionary in his thought, listening to the lectures of Hegel, then a famous professor in Berlin. Hegel was a representative of Prussian reaction. He used his principle of the identity of reason and reality to support the existing powers—for what exists, is reasonable—and to defend the idea of the absolutistic state (an idea called, to-day, "totalitarianism"). Marx, who admired him, but who was of very different political temperament, was in need of a theory on which to base his political opinions. We can understand that it was, for him, a rather fascinating discovery to find that Hegel's own dialectical philosophy could easily be *turned against its own master*—that dialectic is in favour of a revolutionary rather than of a conservative and apologetic political theory. Besides this, it fitted excellently his need, not only for a revolutionary, but for an optimistic theory—a theory forecasting progress by emphasising that every new step is a step upwards.

This discovery, although definitely fascinating for a pupil of Hegel and in an era dominated by Hegel, has lost in our day together with Hegelianism any significance, and can hardly be considered from the present viewpoint to be more than a clever joke of a brilliant young student, revealing the weakness of the speculations of his undeservedly famous professor. But it became the theoretical basis of what is called "Scientific Marxism". And it helped to turn Marxism into a dogmatic system preventing the scientific development of which it might have been capable. Thus, Marxism has remained for decades in its dogmatic attitude, repeating even the same arguments against its opponents which were originally used by its founders. And it is sad but illuminating to see that orthodox Marxism recommends, officially, even to-day, as a basis for the study of scientific methodology, the reading of Hegel's *Logic*—an entirely obsolete book. It is as if one were to recommend Archimedes' mechanics as the basis of modern engineering.

The whole development of dialectic should be a warning against speculative philosophy. It should remind us that philosophy must not be made a basis for any sort of scientific system and that philosophers should be much more modest in their claims. For their task, which they can fulfil quite usefully, is the study of the methods of science.

III.—EPISTEMOLOGY AND THE LOGICAL SYNTAX OF LANGUAGE.

By H. D. Oakeley.

In the following article it will be maintained that the chief paradoxes of the *Logical Syntax of Language* may be traced to the neglect of epistemology. The references will be almost entirely to the works of Prof. Rudolf Carnap. Prof. Carnap himself speaks of epistemology as containing "psychological questions" concerning "the procedure of knowledge, that is the mental events by which we come to know something",[1] and logical questions, *i.e.* questions which are found on analysis to belong to syntax. Thus there are in his view no specifically epistemological problems. In Dr. Wittgenstein's definition—"Erkenntnistheorie ist die Philosophie der Psychologie",[2] there appears to be some recognition that the problem of knowledge goes beyond psychology. Mr. Bertrand Russell says in his Introduction to the *Tractatus*, "There is the problem as to what is the relation between thoughts, words or sentences and that to which they refer or mean: this problem belongs to epistemology". This way of stating the epistemological question whilst admitting its existence, seems to imply a somewhat too objective conception of the problem of knowledge. The relation of thought to its object does not appear to be indicated as the primary and fundamental relation, since thoughts are taken together with words and sentences, as certain objects related to others. In Carnap's reference to epistemology, there is no hint of the question of the relation of all methods of knowledge and logical forms to the knowing subject, as knowing.

An attempt will be made in the present article to show that the identification of the structure of reasoning, or forms of thought, with the syntax or grammar of language, and the treatment of the elements of language, *qua* elements of logic, the signs, words, or symbols, as devoid of meaning or content are alike due

[1] *Philosophy and Logical Syntax*, III, 5.
[2] *Tractatus Logico-Philosophicus*, 4, 1121.

to the disregard or dismissal of this genuine epistemological problem, the relation of all objects to the subjective activity. For the present purpose the wider aspects of this problem need not be raised. It is the special problem of the relation to a subject of the thought-constructs and forms of language which is in question. From this point of view we take the philosophical as distinct from the scientific standpoint in our thinking about objects, when we contemplate them consciously as objects of knowledge. Without asking whether the fact that objects are related to a subject should never be far from consciousness as cognitive, it seems at least essential to remember this relation when we are enquiring into the nature of language and its elements, words, symbols, structure, sentences. To what order of things does the sign belong? In his Presidential address (1937) to the Aristotelian Society, Mr. Russell observed: "They" [the Logical Positivists] "seem to think we can know all about words without knowing anything about facts. . . . The necessary mysticism is not as yet developed. Neurath can still say that a word consists of air-vibrations or heaps of ink. But when vibrations or heaps make up a word, they suddenly become knowable in a way in which otherwise they would not be. Why?"[1] The word, as Russell points out, is a fact like any other thing. The point which I wish to emphasise is that the word as an element in language is a very special kind of fact, though not in the sense of the thinkers to whom Russell referred, but as a thought-thing or mind-constructed thing. As distinct from the facts of nature, and also from ordinary man-made artificial things, it is a thing united with thought by a thinking agent. If we abstract from it its thought or meaning it falls back into nature whether as a mere noise or as air-vibrations, or as altered by man into printer's ink. It can only be given a place amongst empirical things under qualification by its special relation to a subject. As a construction of a subject or subjects, through attribution of meaning to a noise, shape, etc., for purposes of expression and communication, it has a kind of dual nature from which that which it represents can never seem far distant. We may perhaps term it an epistemological atom.

In illustration of Prof. Carnap's anti-epistemological approach to his *Logical Syntax of Language*, the doctrine that the syntax of a language can be formulated within itself is perhaps the best introduction. "It is," to quote Prof. Stebbing, "Professor Carnap's main contention that all sentences expressing principles of mathematics or logic and, further, those expressing principles

[1] "On Verification," *Proceedings of the Aristotelian Society*, 1937-1938.

of the logic of science are syntactical or quasi-syntactical." [1] (He does at times refer to the syntactical method as applicable to all languages whatsoever.[2]) Carnap asks,—" How are these sentences, definitions and rules " (*i.e.* of the syntax of a language) " themselves to be correctly expressed ? Is a kind of super-language necessary for the purpose ? And again a third language to explain the syntax of this super-language, and so on to infinity ? Or is it possible to formulate the syntax of a language within that language itself ? " [3] His reply is that this is possible without any danger of contradictions or antinomies emerging. It seems to me that this view, in connexion with Carnap's conception of the relation of logic to language, throws light on a fundamental inconsistency which pervades the argument of his whole treatise. From the point of view of knowledge with understanding of the methods employed and their claim to validity, which goes beyond the question whether the sentences are correctly expressed, a super-language does certainly seem to be required, *viz.* a language expressive of thought. For no ground for accepting without critical inquiry certain syntactical structures actually in general use, as logical structures, has been given except that greater precision will thus be achieved. Study of his work suggests that too heavy a price may be paid for precision. " The development of logic during the past ten years has shown clearly that it can only be studied with any degree of accuracy when it is based, not on judgments (thoughts or the content of thoughts) but rather on linguistic expressions." [4] Actually, of course, Carnap is using language expressive of thought of great precision and accuracy throughout his work. But on account of his logical theory he uses it under limitations which make it difficult for him to escape the contradiction in which he is involved, *viz.* that the structure of a language without meaning cannot be made intelligible by language without meaning, whilst if meaning is required in the super-language, it is required in the " object-language " which is *ex hypothesi* to be explained. For it is *ipso facto* shown to be inherent in explanation, which is an essential function of language. Carnap seems to approach a somewhat hesitating recognition of this in his discussion of the

[1] R. Carnap, *Logische Syntax der Sprache* and other works; L. S. Stebbing, Critical Notice, MIND, October, 1935.

[2] See Part V, § 78, *Logical Syntax of Language.*

[3] *Ibid.*, Introduction—translated by Amethe Smeaton (Countess of Zeppelin). My references are to the English Edition in which several new sections are added (see Preface, 1936). Where the title is not given the references are to this work.

[4] *Ibid.*

problem of interpretation.[1] It might perhaps be held that the question whether a syntax of syntax is demanded goes beyond what can be required. Some ultimate principles must be postulated. But if such principles are identified with the syntax or the grammar of language some further justification of them does appear to be necessitated. It could not, however, be supplied on account of the contradiction which has been pointed out.

Carnap's answer to the difficulty, *viz.* that it is possible to formulate the syntax of a language within itself, receives the following illustration. "A syntactical sentence," he says, "may sometimes speak about itself, or more exactly about its own design—for pure syntax of course cannot speak of individual sentences, as physical things, but only of designs and forms." For instance, a sentence $\mathfrak{S}$ / states "a sentence of the design — — is closed (or open, demonstrable, synthetic, etc.), and here $\mathfrak{S}$ / itself possesses the design which is described in it".[2] The sentences of pure syntax belong to Language I in Carnap's system, and do not go beyond arithmetic. In Language II, the sentences of which belong to descriptive syntax, extralogical, or going beyond arithmetic, "a sentence can refer to itself in an even more direct manner, *viz.* not merely to design, but to itself as a physical thing, consisting of printer's ink".[3] It is very difficult to see how the formulation of the syntax can be carried out by the reappearance, at certain positions on a piece of paper, of an expression which is the sentence itself. The epistemological problem which might seem as it were to be here struggling for statement in a logic which is a syntax of language, is that of the ground or reason for the structure and forms of the language. This problem has appeared in various forms in the history of philosophy. Mr. Russell, approaching it from the other side, says, "The influence of language on philosophy has, I believe, been profound, and almost unrecognised. The subject-predicate logic with the substance-attribute metaphysic, are a case in point. It is doubtful whether either would have been invented by people speaking a non-Aryan language".[4] Referring to Russell's view, F. P. Ramsey asserts that "nearly all philosophers, including Mr. Russell himself, have been misled by language in a far more far-reaching way than that"; "the whole theory of particulars and universals is due to mistaking for a fundamental characteristic of reality what is merely a

[1] Part IV, §§ 62, 64. See page 442 *seq.* of the present article.
[2] Part III, § 35. [3] *Ibid.*
[4] *Contemporary British Philosophy*, First Series.

characteristic of language ".[1] It may, however, be questioned whether language could force an interpretation of the nature of things upon philosophic reflection if there were not some basis for the linguistic forms other than that men have stumbled into them in the course of their long struggles for intelligible communication. Whether or not the theory of particulars and universals has anything to do with " reality ", the history of philosophy seems to show that its origin and basis are to be found in the nature and methods of our thinking about experience, and that it is this—whatever its validity—which has impressed itself upon language. No doubt in return, the forms of language have reacted upon thought. It is indeed difficult to conceive how the grammatical distinctions or " parts of speech " could have originated except in association with particular experiences and meanings, whatever the worth of this experience and its interpretations from a philosophical point of view. Does Prof. Carnap regard the syntactical forms as *a priori?* This might seem to be implied in his position, and theory of formal logic or pure syntax as the only logic in a strict sense. His treatment, however, of the " rules of formation and transformation " of a language in relation to the " Axiom system " (*e.g.* of geometry), whilst it seems to imply that these rules are equally primitive with the axioms, appears to assume them without explaining the ground for regarding them as needing no proof of their necessity.[2] It is not, however, clear how we should conceive an *a priori* of *language.* Not the least perplexing aspect of Prof. Carnap's argument is that he evidently assumes himself to be in the direct line of the formal logicians since Aristotle, and does not seem to recognise the gulf which exists between the theory of logic as concerned with the forms of thinking, and as identified with the syntax of language. In this respect he appears to have departed from the philosophy of mathematical logic, as developed by his most eminent predecessors. In the *Logical Syntax* much emphasis is laid on the fact that from the form alone of a word-series in a well-constructed language, the rules of formation of a sentence can be apprehended without any knowledge of the meaning. Thus in the imaginary sentence

" Pirots carulize elatically " [3]

we can see in the form alone of the expression that we have here a substantive, verb, and adverb. Also that the sentence " A carulizes elatically " can be " deduced " from the combination

[1] *Foundations of Mathematics.*
[2] Part IV, § 71, *e.*
[3] Introduction.

problem of interpretation.[1] It might perhaps be held that the question whether a syntax of syntax is demanded goes beyond what can be required. Some ultimate principles must be postulated. But if such principles are identified with the syntax or the grammar of language some further justification of them does appear to be necessitated. It could not, however, be supplied on account of the contradiction which has been pointed out.

Carnap's answer to the difficulty, *viz.* that it is possible to formulate the syntax of a language within itself, receives the following illustration. "A syntactical sentence," he says, "may sometimes speak about itself, or more exactly about its own design—for pure syntax of course cannot speak of individual sentences, as physical things, but only of designs and forms." For instance, a sentence $\mathfrak{S}$ / states "a sentence of the design — — is closed (or open, demonstrable, synthetic, etc.), and here $\mathfrak{S}$ / itself possesses the design which is described in it".[2] The sentences of pure syntax belong to Language I in Carnap's system, and do not go beyond arithmetic. In Language II, the sentences of which belong to descriptive syntax, extralogical, or going beyond arithmetic, "a sentence can refer to itself in an even more direct manner, *viz.* not merely to design, but to itself as a physical thing, consisting of printer's ink".[3] It is very difficult to see how the formulation of the syntax can be carried out by the reappearance, at certain positions on a piece of paper, of an expression which is the sentence itself. The epistemological problem which might seem as it were to be here struggling for statement in a logic which is a syntax of language, is that of the ground or reason for the structure and forms of the language. This problem has appeared in various forms in the history of philosophy. Mr. Russell, approaching it from the other side, says, "The influence of language on philosophy has, I believe, been profound, and almost unrecognised. The subject-predicate logic with the substance-attribute metaphysic, are a case in point. It is doubtful whether either would have been invented by people speaking a non-Aryan language".[4] Referring to Russell's view, F. P. Ramsey asserts that "nearly all philosophers, including Mr. Russell himself, have been misled by language in a far more far-reaching way than that"; "the whole theory of particulars and universals is due to mistaking for a fundamental characteristic of reality what is merely a

[1] Part IV, §§ 62, 64. See page 442 *seq.* of the present article.
[2] Part III, § 35. [3] *Ibid.*
[4] *Contemporary British Philosophy*, First Series.

characteristic of language ".[1] It may, however, be questioned whether language could force an interpretation of the nature of things upon philosophic reflection if there were not some basis for the linguistic forms other than that men have stumbled into them in the course of their long struggles for intelligible communication. Whether or not the theory of particulars and universals has anything to do with " reality ", the history of philosophy seems to show that its origin and basis are to be found in the nature and methods of our thinking about experience, and that it is this—whatever its validity—which has impressed itself upon language. No doubt in return, the forms of language have reacted upon thought. It is indeed difficult to conceive how the grammatical distinctions or " parts of speech " could have originated except in association with particular experiences and meanings, whatever the worth of this experience and its interpretations from a philosophical point of view. Does Prof. Carnap regard the syntactical forms as *a priori?* This might seem to be implied in his position, and theory of formal logic or pure syntax as the only logic in a strict sense. His treatment, however, of the " rules of formation and transformation " of a language in relation to the " Axiom system " (*e.g.* of geometry), whilst it seems to imply that these rules are equally primitive with the axioms, appears to assume them without explaining the ground for regarding them as needing no proof of their necessity.[2] It is not, however, clear how we should conceive an *a priori* of *language*. Not the least perplexing aspect of Prof. Carnap's argument is that he evidently assumes himself to be in the direct line of the formal logicians since Aristotle, and does not seem to recognise the gulf which exists between the theory of logic as concerned with the forms of thinking, and as identified with the syntax of language. In this respect he appears to have departed from the philosophy of mathematical logic, as developed by his most eminent predecessors. In the *Logical Syntax* much emphasis is laid on the fact that from the form alone of a word-series in a well-constructed language, the rules of formation of a sentence can be apprehended without any knowledge of the meaning. Thus in the imaginary sentence

" Pirots carulize elatically " [3]

we can see in the form alone of the expression that we have here a substantive, verb, and adverb. Also that the sentence " A carulizes elatically " can be " deduced " from the combination

[1] *Foundations of Mathematics.*
[2] Part IV, § 71, *e.*
[3] Introduction.

of the first sentence with the sentence " A is a pirot " (according to the rule of transformation). " In order to determine whether or not one sentence is a consequence of another no reference need be made to the meaning. It is sufficient that the syntactical design of the sentence be given." Here there would seem to be an oversight of an almost elementary character due to the reluctance to allow historical conditions to obtrude themselves. A combination of words, unknown in any language, may nevertheless (it is presumed) show a linguistically correct structure. Since the words have no meaning, the only source of order in their arrangement seems, at first sight, to be their order in the combination. But the structure would remain if the order were altered. " Carulize pirots elatically ", etc. Why is this ? Because meaningful words have often been intelligibly combined with similar endings. Words continue to convey the meagre fragment of meaning which their form suggests. The only significance suggested by our familiarity with actual languages, is that whatever pirots may be they belong to a world in which things may behave in a way here described as carulizing, in a manner here termed elatically. Whatever the meaning of the two expressions the series of words is only saved from being pure gibberish because these associations are suggested by experience. Carnap seems to be assuming that the structure of the parts of speech is a form which can give validity to the sentence, whatever the meaning of the terms, on the analogy of an algebraic statement. There appears to be here and elsewhere some ambiguity in his view of the logical character of empirical sentences. If the signs which look like words in the apparent sentence " Pirots carulize elatically " were replaced by mathematical symbols, and the sentence had the form of an algebraic statement, then the form would be sufficient to give the symbols their place in the statement. The algebraic statement is valid for any one of an indefinite number of values of the variable.[1] The empirical and concrete statement is only valid for one kind of thing which has a determinate nature in all its instances. In the assumption that some definite sentence may be constructed by using the structure of the syntax without employing terms with meaning, is there not a failure to recognise the essential difference between mathematical form and symbol, and the form and signs (words) of the empirical sentence ? " Pirots " is a no-sign, neither word nor symbol. The symbol performs its part without determinate meaning. The word must

[1] Whether the variable is taken as restricted or unrestricted does not, I think, affect the point that Carnap seems, consciously or unconsciously, to apply the idea of algebraic form to the empirical sentence.

have this, or the sentence falls outside language. It is evident that the syntactical structure which Prof. Carnap has in mind in his references to actual languages is that of the languages belonging to the Aryan group. In objection to the statement of C. I. Lewis that "Inference depends upon meaning, logical import, intension", he observes: "It is theoretically possible to establish the logical relations between two sentences (consequence, compatibility, etc.) in Chinese without giving their sense provided that the syntax of the Chinese language is given. In practice this is only possible in the case of the simpler artificially constructed languages." [1] Why is it not practically possible in the case of the Chinese language? The reason surely why it is neither practically nor theoretically possible unless the meaning be given, is that since the Chinese language is not inflectional its syntax does not supply grammatical forms by differences in the word characters or endings but is only indicated by the position of words in the sentences and—in the spoken language—their intonation or modulation. Here I am referring to accounts by authorities of the grammar and syntax of the Chinese language as I do not know it myself.[2] The same word in Chinese, we are informed, may represent in turn nearly all parts of speech. According to Julien, "The inflections of nouns and verbs which give so much precision to the Aryan languages find up to a point their equivalents in the mobility of the Chinese signs which acquire every sort of grammatical value according to the place they occupy in the phrase". Perny observes, "The rule of the position of the words is the key of the Chinese language. . . . By means of the placing and disposition of words in the discourse one and the same word can, whilst remaining invariable, become successively adjective, verb, active, passive, neuter, and even adverb". It would appear that in Carnap's view this character of the Chinese syntax would not in principle prevent the Chinese sentence from being "autonomous", or independent of its meaning. But is not the admission that it is not "practically possible to establish the logical relations without giving the sense", in the case of the language of a highly civilised people, sufficient to show that logic cannot be identified with the syntax of language without reference to the meaning? The fact that there are such languages confirms the view that the structure of the sentence, noun, verb, etc.—has no status, reason or law in abstraction from meaning and thought. The fundamental divergence of

[1] Part IV, § 71.

[2] *Grammaire de la Langue Chinoise, Prolégomènes,* Chapter I, Perny. *Syntaxe nouvelle de la Langue Chinoise,* Julien.

syntactical form does not appear to interfere with common understanding and communication through translation. This is due to the fact that it is not a divergence in the forms of thinking.

Determined by his presuppositions to adopt the language forms as the original facts with which we have to do, Prof. Carnap seeks in the elements of language, words, expressions, sentences, their own explanation. We have therefore the autonymous sentence, and the non-designating design. A symbol used as a name for its own symbol-design is " autonymous ". Hence it is an important question whether we are using Gothic, Roman, or Greek letters as symbols, because it is of the design that we are speaking. In descriptive syntax it is admissible to speak of that which is designated. But to consider " the question what it is that is designated by a sentence (some people say thoughts or the content of thoughts, others facts or possible facts)—is a question that easily leads to philosophical pseudo-problems ".[1] Carnap therefore does not consider it. The assumption that the nature of the sentence of pure syntax consists only of its design seems to involve a fallacy which is akin to the fallacy of substituting means for end in ethics. As already noticed, " meaning ", "thoughts ", " facts ", etc., appear to have, if not dictated, at least profoundly influenced the forms and the signs of languages. If the words and expressions are taken apart from this relation they cease to be language. Yet there remains an appearance of language and of talking about something, or reference to objects. It is asked whether we wish to talk about things designated, or referred to, or about the sentences and designs. For example, it is apparently worth while to say, " If a character in ink having the figure of an upright ellipse occurs at a place, then, no character consisting of one vertical and three horizontal strokes occurs at that place ".[2] But what we talk, or write about must have some meaning, this being inseparable from language. If then we talk about talking, use language about language, we are making our one series of terms perform a double function. The second function, that of meaning, which has been removed from the sentence but without which language cannot live, it must recreate out of itself. Thus the word or symbol becomes its own meaning. When meaning then is expelled from language the empty forms and words make of their own reflection or shadow new objects, and the sentence is about itself or its own design.

The modern discussion of the relation of signs to meaning goes back to Prof. Stout's article on " Thought and Language ",[3]

[1] Part IV, § 69. [2] Valid descriptive sentences, Part II, § 25.
[3] MIND, April, 1891.

though it has in some respects departed from his standpoint. Prof. Whitehead refers to it in his *Universal Algebra*,[1] and Prof. Stebbing in her *Modern Introduction to Logic*.[2] It is only necessary here to recall Stout's view of the signs of language as "expressive signs", in which the word is not attended to in its own interest, and of the "substitute signs" of mathematics. "The more perfectly the word discharges its function as a sign, the less conspicuous and impressive will it become as a particular presentation." "Algebraical and arithmetical symbols, as also the symbols of formal logic, are to a great extent used as substitute signs," which are "counters which take the place of meaning". On a first inspection it might appear that in Carnap's system the words or symbolic letters of language are used as substitute signs in Stout's sense. But on a further examination of Carnap's analysis of language it appears that at least when he is treating it in the strictest sense as expressing the syntax which is logic, the signs cannot be described as substitutes for meaning because they have no connexion with meaning. Even the substitute signs of mathematics do not seem wholly devoid of reference beyond themselves, though this is dismissed from thought during the calculation. They remain *substitutes* without complete independence. As regards the symbolic logic of Russell and others among its chief exponents, though they go far beyond Aristotle and the traditional formal logic in the development of symbolism, the formal structures appear to be primarily forms of thinking rather than of language conceived as independent or "autonomous". If a non-mathematician may express an opinion, the mathematical symbol itself does not appear to be identical in function with the symbol of non-mathematical language, because its place in the mathematical calculus gives it a different part to play. According to Whitehead—"The rules" [of the calculus] "should be such that the final state of the signs after a series of operations according to rule denotes when the signs are interpreted in terms of the things *for which they are substituted*,[3] a proposition true for the things represented by the signs."[4] Such a return to the things represented is not, it seems, permitted in Carnap's logically valid language. It is doubtful whether it is permitted in any language he recognises, without this becoming a-logical. For even "the sentences of the logic of science are formulated as syntactical sentences about the language of science".[5] He indicates (§ 73) his divergence from Wittgenstein's view as

[1] Vol. I, Chapter I.
[2] Part I, Chapter VIII, § 1.
[3] Italics mine.
[4] *Universal Algebra*, Vol. I, Chap. I.
[5] *Logical Syntax of Language*, Part V, § 74.

interpreted by Schlick—that the sentences of the logic of science go beyond the formal and refer to the sense of the sentences and terms and "The content, soul, and spirit of science naturally consists in what is ultimately meant by its sentences". Whatever the ground for applying the term "calculus" to logical methods, other than those which properly belong to mathematics, to speak as Carnap does of *languages* as calculi seems inadmissible. For even when the language expresses a chain of reasoning it is not the language as language, but the reasoning to which the term calculus might be appropriate. According to Whitehead a calculus is "The art of the manipulation of substitute signs according to fixed rules, and of the deduction therefrom of true propositions".[1] As has been pointed out, in Carnap's system the language signs cannot truly be said to belong to the class of substitute signs. In gathering up the crumbs which fall from the rich table of the mathematician, the linguistic logician destroys their value as food for a process of thought, since the symbols in his treatment of them cease to be symbolic. It is evident that the doctrine in regard to the objects of logic, involved in the *Logical Syntax of Language*, tends to a renunciation of the idea of universality in the principles of thinking. Perhaps this is implied when we are told that the process of thought is the concern of psychology. Reference has been made to the fact that the syntax of the Aryan languages differs fundamentally from that of non-inflectional languages, in particular Chinese. Yet greater differences in instruments of expression and communication suggest themselves when we reflect that there seems to be nothing in principle to prevent the use of means *toto genere* distinguished from the accepted forms of language. "The natural signs of gesture language, the conventional signs of the manual language taught to deaf mutes, and all kinds of written language as well as articulate speech," are included by Prof. Stout[2] amongst expressive signs. It is not difficult to imagine that certain art-forms if they could have been cultivated, consciously and unconsciously, for the purpose of intercourse and communication by means in any way comparable to those through which languages have been continuously developed throughout human history, might have become adequate to the exchange of thought and conveyance even of abstract ideas up to a high pitch of success. However numerous and diversified may be the kinds and structures of signs through which thought, content, meaning, might conceivably be conveyed,—and from the standpoint of a subject of knowledge whose experience is primarily sensuous, no

[1] *Universal Algebra*, loc. cit. [2] *Loc. cit.*

theoretic limit seems necessarily final—the same methods of thought and forms of significance would in principle be expressible. But if the expression were all, and no meaning directly expressed there would only be, if language at all, an anarchy of languages. The unity of science, the attainment of which is as Mr. Max Black points out, one of the main objects of Carnap's work,[1] would seem very remote.

A further question suggests itself in regard to the limits of the logical syntax of language, *viz.* whether it can, as Prof. Carnap certainly seems to assume, be applied to spoken, as well as written language.[2] A difference between written and spoken language which seems highly relevant to this question is that it is only in the written, or more precisely in the printed (or otherwise mechanically presented) language that the medium which appears indispensable to the theory can be found—*viz.*—a means for securing and preserving the sameness for perception which seems essential if the whole concern of logic as pure syntax—formation and transformation of sentences, is with the designs as such. How can we know, for instance, that we have a case of consequence or derivation before us, transformation of one sentence or series of sentences into another, if the designs of the sentences as series of words or symbols vary as does the pronunciation or the handwriting of different individuals? A possible reply that the signified (or type) designs are the same and the differences of the "token" designs is immaterial, will not avail on Carnap's view, as is evident, for instance, in the importance attached to the differences between Gothic and Roman letters. A point of interest here is that it would appear from these considerations that the invention and general use of printing or other mechanical methods to secure exact similitude in the signs, must be assumed before Prof. Carnap's system could be established. As regards spoken language, Mr. Russell draws attention to the perplexities of the treatments of words on the theories of Carnap and Neurath in connexion with the differences between the spoken and the written word.[3] One may ask, for instance, how it would be possible to express in speech the explanation for the rules of transformation for Language I. This is based on the position of letters on the page—*e.g.* "Let s be TN_{z1}, *stfrei* (k, s, x) is the position number of the $(k+1)^{th}$ z_1

[1] *The Unity of Science*, R. Carnap, Introduction by M. Black; *cf.* also *Philosophy and Logical Syntax*, III, 9.

[2] The fact that the word *Sprache* is used for both language and speech somewhat obscures this question.

[3] "On Verification."

(counted from the end of the expression x) which occurs freely in x O in the case where there are not $k+1$ free z_1 in x), *anzfrei* (s, x) is the number of the z_1 which occur freely in x. $Sb\,(k, x, s, y)$ is that expression which results from the expression x when starting from the last free z_1[th] k last free z_1 of x are successively replaced by the expression y ".[1]

How will all this be expressed in speech ? The rules refer to a subject matter so particularised,—commas, inverted commas,—letters, Roman, Gothic, Greek, of different sizes, that the sentences could hardly have their equivalent in speech, or only with very laborious utterance. Or how would the explanation be remembered if abstracted from meaning ? In the case of speech the following difficulty of a philosophical character seems also of importance. The fact that the series of words occurs in a succession which is temporal, is much more obvious in the spoken language since each vanishes before its successor arrives. The signs of the written language may seem to have some degree of permanence. They endure and can be contemplated as possessing constancy of form. In so far as there is no reference to meaning, they may be regarded as presenting a kind of static order. This property seems to render them more amenable to the methods of the *Logical Syntax* than are the expressions and sentences as spoken. Yet every natural language bears the marks of its origin to such a degree that when the sentence is treated as autonomous and the words merely as designs, the connexion and interrelation of these designs seems quite arbitrary, and the rules of formation and transformation unexplained. Language originated as the creation of rational beings in the necessities of their temporal experience, as is evident, for example, in the tenses of the verb. The inherent tendency of the mathematical logic is in the direction of diminishing the impurity introduced into reasoning by the time-factor. But the logician who treats language as independent of thought does not appear to be entitled to take advantage of this characteristic of mathematics. In so far as the attempt to make the structure of language superior to temporal forms is successful, it ceases to be language. A language can express, or at least turn our thinking towards, the idea of the timeless, with success proportionate to its resources and flexibility. But it cannot in itself and in abstraction from meaning and content exhibit a non-temporal form. Language as language consists of events. As pointed out above, this may be partly concealed in the written form. But even here it is evident on further attention. The written sentences may, if their re-

[1] Part II, § 23.

presentative function is admitted, constitute the expression of a series of objects, but the expressions as such are events, and betray in their form the fact that writing is secondary to speech. It may be asked, Is this a malady of language, and is the arithmetisation of syntax proposed as a cure ? Prof. Carnap's reasons for arithmetisation are not easy to understand. He appears to regard it as a great advantage for the language of science. But the chief examples he gives hardly seem to justify this claim. " In the case of a finite class of colours which are to be expressed —blue, red, etc., these predicates can be replaced by a single functor colour, by numbering the colours in some way and stipulating that col. $(a) = b$ shall mean—the position a has the colour no. b. Thus, in general, the values of the one functor (zei) are correlated to the different symbol-designs partly arbitrarily, and partly in accordance with certain rules. The values are called the term numbers of the symbols." Again he says, " The establishment of term numbers for the different symbols *can be effected arbitrarily* ".[1] In this way it is supposed that all the sentences of pure syntax can be shown to be " analytic sentences of elementary arithmetic ". It is difficult to see that this arithmetisation has scientific value, where it is " arbitrary " and does not refer to empirical fact, for instance (in the example of the colours), to the order of colours in the spectrum. In another instance,—that of using temperature numbers for the " inexact designations, warm, cool ", etc., Carnap does point out that by this method the formulation of universal laws (for example that of the relation between temperature and expansion, or pressure) is rendered possible. But his statement that " The mathematical sentences considered from the point of view of language as a whole are only aids to operations with empirical, *e.g.* non-mathematical sentences ", hardly seems to receive sufficient proof. It can only be pointed out here that in general the method of mathematising the sentences related to empirical fact, and treating them wholly from the extensional standpoint, would seem to remove the formulation of laws of nature further and further away from expression of the individuality of things and their processes. Yet as Mr. Joseph points out,[2] " The fact is that logistical theory " [in which I presume he would include the *Logical Syntax of Language*] " rejects general knowledge. ' The world ', says Dr. Wittgenstein, ' is completely described by the

[1] Italics mine.

[2] " A Defence of Freethinking in Logistics," MIND, Oct., 1932. Published, of course, before Prof. Carnap's *Logische Syntax der Sprache*.

specification of all elementary propositions plus the specification which of them are true and which false'".

On the question which provinces of language are outside logic, or a-logical, which is obviously of great importance, Prof. Carnap is somewhat ambiguous. He distinguishes languages "L" and "P" as the language (L) whose rules are strictly logico-mathematical, and that (P) which admits "extralogical rules of transformation", including among the primitive sentences the so-called laws of nature, *i.e.* universal sentences of physics (in the widest sense). "It is possible to go even further, and include not only universal but also concrete sentences—such as empirical observation-sentences."[1] It seems clear that observation-sentences in this connexion do not belong to logic proper, yet they are assigned a very important part to play in science. We are told at the outset[2] that in addition to syntax which is limited to the formal aspect of language, any particular language has other aspects, *e.g.* the meaning of words studied in semasiology, their relation to actions and perceptions in psychology and so on. In general "the science of language investigates language from all these standpoints". Can it do this without logic? This seems to raise the curious question whether logic can have as its object the a-logical. The answer would seem to be yes, from the non-linguistic standpoint, as rational thinking would otherwise be cut off from a great part of experience, except on the absolute idealistic view of the world as solidified logic, in which case, of course, there would be no a-logical object. But if the proper object of logic, as logical syntax of language, is only language, it would seem the extra-logical aspects of language must be outside its purview. There are no synthetic logical sentences.[3] In general, Carnap seems to agree with Bertrand Russell, in not admitting empirical logic to a place in logic in the strict sense. Russell observes that he does not distinguish between inference and deduction. "Induction appears to me either disguised deduction, or a mere method of making plausible guesses."[4] In Part V, § 72, of his *Logical Syntax*, Carnap distinguishes a "non-logical proper object domain", with which object questions have to do, *e.g.* in the domain of zoology, the properties of animals, relations of animals to one another, etc., whilst the logical questions refer to sentences, terms, theories, etc. "The logic of science is nothing more than the syntax of the language of science." Yet can the conclusion be avoided that at the pure logical level

[1] Part IV, § 51. [2] Introduction, § 2.
[3] Part IV, 523 Theorem.
[4] *Principles of Mathematics*, Part I, Chapter II, note.

the facts of the " non-logical proper object domain " are referred to at a second remove ? Where have the sentences, terms, theories, been formulated, whence do they derive ? Evidently from the object domain. Ultimately it is from this that their character proceeds. Is this consistent with Carnap's position ? It does not seem in conformity with the tendency he frequently shows to regard the elements of language as prior to things. A curious example of this is his suggested explanation of the " origin " of the " material mode of speech ". His characterisation of this as " a special kind of transposed mode of speech " indicates that it is secondary to the more correct mode. Instead of saying something about a word or sentence " we say something parallel about the object designated by the word (or the fact described by the sentence) ". Carnap suggests as a psychological explanation that " the conception of the substituted object *b* is for some reason more vivid and striking, stronger in feeling-tone, than the conception of the original object *a* ".[1] *The original object is the word.* The assumption that people want to talk about words but through some psychological peculiarity find themselves talking about things, gives an impression of that naïveté which sometimes veils an extreme subtlety. That he is referring not to experts in logical syntax (though he admits that these also fall into the " material mode ") but to plain persons is obvious from many of his examples of the material mode,—as, for instance, the favourite one of the lecture on Babylon, which may only be spoken of as a lecture in which the word Babylon occurred (thus giving a very inadequate description for the unsuspecting plain man). The doctrine of language itself as the original object to which language is directed receives a further confirmation in the view of its " autonomy ", and independence of that which is designated. For how could language or rather the many languages, natural and artificial, have acquired this status if words and symbols are truly signs, and syntax lacked that character of priority which is assigned to it ? There is also, however, the question, how could languages have any relation whatever to the facts which the designs, shapes, or noises do not represent ? But if they do have some inexplicable relation to things, does it not follow that none of these things as related to the symbols and structures of the different languages can be the same things which are variously expressed in all ? For the signs, and in some language-groups the syntactical structures, are not the same, and these are the objects of the " object language ", *i.e.* the objects with which we primarily have to do in speech and

[1] Part V, § 80.

writing. Perhaps in Prof. Carnap's attempt, very remarkable from his standpoint, to show that the interpretation of a language can be "formally represented", a way out of this anarchy may be found. In spite of his principal arguments to the conclusion that language with no relation to meaning and content can and does function as the essential instrument of logic, he recognises the crucial question of the possibility of communication, which might be termed the alpha and omega of language. For even the solitary thinker is communicating with himself, and could not long continue this without language in some form. "When only the formal rules of a language are known, it is not possible to use it as a language of communication because the *interpretation* is lacking."[1] For a solution he turns to the fact of translation of one natural language into another. "The interpretation of a language is a translation and therefore can be formally represented; the construction and examination of interpretations belong to formal syntax." I find Carnap's explanation of the method by which, as he argues, "a rendering of the sense or meaning" in another language is secured, somewhat obscure. But it seems at least clear that the question whether the sense of the French sentences has been rendered in the German sentences, which signifies that "the condition of being in agreement with the actual historical habits of speech of the people in question has been fulfilled", is "historical and empirical" and "lies outside the domain of pure syntax". How then is the meaning "formally represented"? This is, it appears, secured because we know that the construction of the syntax of any individual language means "the construction of a calculus which fulfils this condition". It appears to me that the essential condition is simply assumed because within the formal interpretation it cannot be shown. It is assumed that at the original event of the construction of a language, the syntax was so constructed as to be related to the sense, meaning, or "historical habits of speech". "Content" (it should be added) is earlier defined (§ 49) as "the class of the non-valid sentences which are consequences of a certain sentence". (This is not enlightening, since content would thus be extra-logical.) In a language of natural growth it must be the case that the syntax through gradual development *is* related to sense or meaning. But this original relation is not admitted in Carnap's main argument, and he does not therefore seem justified in assuming it in order to explain interpretation and translation within the limits of "formal representation". Does not consistency demand that the problem of interpretation

[1] Part IV, § 62.

should be presented as that of finding the interpretation of a primal language of which no translation is possible since *ex hypothesi* no other language is available? On Carnap's view there ought to be such a language prior to interpretation, or ascription of meaning. If we assume this disembodied or rather disensouled language to exist, how is it to acquire the soul it lacks? Although this hypothetical case is not really conceivable because prior to union with meaning there would not even be a shell of language, but only formless sounds, or irregular shapes, Carnap seems bound to approach the problem in this way, and face the question of the origin of meaning rather than assume it as already there in translatable languages. For this is to beg the question. The need for epistemological criticism is especially evident in the difficulty of bringing back into language, for the sake of communication, the meaning and content which have been artificially abstracted from it. This abstraction could not have occurred if the relation of language to the subject of knowledge had not been at the outset overlooked. It should perhaps be added that in his criticism of the view of C. I. Lewis that a logic of content or meaning is demanded (as well as an extensional logic), Prof. Carnap seems to go beyond his own position in the section on interpretation, in the dictum "All questions in the field of logic can be formally expressed, and are then resolved into syntactical questions".[1] There is here no reference to the extra-syntactical means of conveying the relation to historical habits of speech.

In conclusion it may be restated that the main principles of criticism from an epistemological standpoint derive from the conception of language as essentially construction for the purposes of knowledge. In this light the elements of language must be regarded as objects only to be understood in relation to a subject, and therefore as having a dual aspect as thing and meaning united. In regard to the laws or rules of language, we come upon the problem of the relation of "convention" to necessity in the constructions or formations through which collections or series of words or signs are combined into those objects, which Carnap terms sentences, in order that they can be presented to intelligence and given their place in an assertible whole. What are we to understand by convention? The idea plays a very important part in Carnap's system as is evident from such observations as the following, exemplifying the principle of "tolerance".

"It is not our business to set up prohibitions but to arrive at

[1] Part II, § 71.

conventions."[1] "Since there are no morals in logic, what meaning can 'admissible' have?" (*e.g.* when inquiring whether indefinite symbols are admissible). "The problem can only be expressed in this way: How shall we construct a particular language? It is a question of the establishment of rules of syntax," etc.[2]

Even the rules of the physical languages depend upon "conventional decisions" according to Neurath.[3] But *quis custodiet ipsos custodes?* The conventions will hardly be arrived at by a common agreement of persons who recognise no common principles of thought underlying the grammar of their compact. The order established by convention requires a prior convention for the co-operation of those who establish it, and this begins an infinite regress which can only be terminated by recognition of primary logical principles, agreed to be necessary. Otherwise the principles of logic would be as conventional as the relation of words to meaning or content. When these principles are treated as rules of syntax or grammar the descent in this direction begins. The belief that there is something super-conventional in the logical forms if they can be regarded as rational would save us from ultimate scepticism. But if logical forms are nothing but syntax, and syntax indistinguishable from grammar, they cannot be shown to be universally accepted. And Carnap abandons universality. "Everyone is at liberty to build up his own logic, *i.e.* his own form of language, as he wishes. All that is required of him is that he must state his methods clearly, and give syntactical rules instead of philosophical arguments." But if not permitted some arguments which are more than syntactical rules, he will have nothing on which to found his rules, and is, by this rule of the game, precluded from showing that his form of language *is* language, as it will certainly not be logic. In sayings of this kind we may see the culmination of modern philosophical scepticism.

[1] Part I, § 17. [2] Part IV, § 44. [3] Part V, § 82.

IV.—DISCUSSIONS.

KANT'S CONCEPTION OF SCIENTIFIC KNOWLEDGE.

1. KANT demands that knowledge which is to be called scientific must satisfy two requirements:

(*a*) it must fit with all other knowledge of its kind into a single complete system which is constructed in accordance with an *a priori* idea;

(*b*) it must be apodeictically certain.

(*a*) A science cannot be a mere aggregate of separate cognitions: it is a complete whole of interconnected knowledge, and for that to be possible the place of each part of it must be determined by a single principle. This is Kant's consistent doctrine, as witness K.R.V., B 860 = A 832, *Metaphysische Anfangsgründe der Naturwissenschaft, Vorrede* (ed. Hartenstein, iv, 357), and Kiesewetter's *Logik*, § 194 (a very clear expression of the Kantian point of view), quoted by Vleeschauwer, *La déduction transcendentale*, t. ii, p. 18, n. 5.

(*b*) Kant also supposes (and here he follows the common rationalist tradition) that scientific knowledge must be absolutely certain. It is this, no doubt, which leads him to his famous declaration (M.A.d.N., iv, 360) that there is just as much science in each special account of nature (*in jeder besonderen Naturlehre*) as there is mathematics. "Sciences" like chemistry (*ibid.*, 361) and psychology (*ibid.*), which are thought by Kant not to be susceptible of mathematical treatment, are accordingly said not to be sciences in the strict sense. (Actually the general Kantian principle here is somewhat puzzling, for, as we shall see presently, mathematics itself cannot be a science on the Kantian account. But the propositions of which it consists are for all that apodeictically certain.)

2. If this is followed the only "sciences" which truly deserve the name are metaphysics, as Kant conceives it to be legitimate (the "transcendental philosophy" of K.R.V., B. 873 = A 845, with its applications) and formal logic (for which see Kant's *Logik, Einleitung*, i). No empirical discipline could be a science, not even those to which mathematics could be applied, for, despite the demand for system which the ideas of reason impose on the scientist (K.R.V., Appendix to the Dialectic), none could give promise of completion. Empirical disciplines depend on the given manifold of intuition, and that means that their results (*a*) are not necessarily determined

by a single principle, and (*b*) are infinite in their number, owing to the unceasing stream of empirical intuition with its constant novelty of situation. Similarly, mathematics could not strictly be a science, for the mathematician too has an unlimited manifold of intuition before him, even if that intuition is *a priori*, and that means that it is always open to him to make new discoveries (though not, I suppose, on a Kantian view, to change the fundamental principles of his study). If that is the case the mathematician could never arrive at a completed whole of knowledge. And, once more, Kant's own philosophy (the " propædeutic " to his metaphysics rather than the metaphysics itself) ought not to be classed as scientific knowledge: for, as the original introduction to the *Critique of Judgment* remarked (ed. Lehmann, p. 3), there is a difference between " the system of rational knowledge through concepts " and " a philosophical investigation of the possibility of such knowledge ". But this was a distinction which Kant, to his misfortune, was apt to blur, and in at least one place he describes the first *Critique* as a " metaphysic of metaphysics " (letter to Herz, Berlin edition, x, 252). This perhaps implies that he thought " critical " philosophy a true science.

3. As we have seen, empirical disciplines cannot, on this account, be granted the dignity of sciences proper. Their conclusions may be couched in semi-systematic form, but in the nature of the case they can hardly rest on an *a priori* idea or possess apodeictic certainty. Now Kant did not apparently feel any difficulty in thus refusing the name of science to chemistry, which he described (M.A.d.N., iv, 360) as " systematic art or experimental doctrine "; whilst he thought the obstacles in the path of a scientific psychology so great that it could never hope to stand even on that level (*ibid.*, 361). But he had serious qualms about denying the scientific character of physical knowledge. Physics was to him quite clearly the premier natural discipline, and it seemed hard to withhold from it the name of *Wissenschaft*. Was physics too to be put down as *systematische Kunst oder Experimentallehre?*

Kant's answer to this was negative, partly because he thought physical problems were susceptible of mathematical treatment,[1] and partly because he believed the study to have a pure or *a priori* part. " Pure " physics at least was truly scientific. It was reached by applying the concepts of metaphysics, alleged by Kant to form a complete system, to the particular sphere of corporeal nature. This resulted in a " metaphysics of corporeal nature ", and this is the same as " pure " physics. Such a " metaphysics of corporeal nature " was sketched by Kant in M.A.d.N. By taking the schematised categories and the principles of the understanding along with the empirical notion of matter, understood in its minimum characterisation as the movable in space, he was able to produce a list of *a priori* principles which both constituted the first founda-

[1] I say nothing more of this in the present paper, perhaps wrongly. But I doubt whether it affects the main issue discussed below.

tions of physics and were themselves scientific in the strict sense. He admitted (iv, 368) that they amounted to little in comparison with the body of empirical laws, but for all that he thought this "metaphysical" part of physics of great importance. He imagined indeed that he was benefiting both physical and metaphysical investigations by working it out (366-367).

4. The difficulty about thus dividing physics into "pure" and empirical parts was the wide gap which subsequently separated them. The first was sharply different from the second, so much so that it was paradoxical even to say that they belonged to the same "science". The recognition of this fact, after the completion of his "critical edifice", led Kant to conceive the idea of strengthening the claim of physics to be a true science by extending its *a priori* part. To effect this he proposed to deduce from the categories not only the general presuppositions of physical inquiry, as he had done in M.A.d.N., but also certain particular laws too, as that the types of force to be met with in experience could only take certain specifiable forms. We have, in the work which began as a "Transition" from the metaphysics of corporeal nature to physics and which is now available in the shape of the *Opus Postumum*, the results of Kant's attempt, marked by an ingenuity and a determination worthy of a better cause, to carry out this proposal. What he was doing in it should not, however, be misunderstood. If M. Vleeschauwer's admirably clear account of the work in the third volume of *La déduction transcendentale* is to be trusted, Kant did not hope to deduce all the particular laws of physics from its *a priori* principles: despite his apparent compromise (if we can rely on M. Vleeschauwer once more) with the "constructivism" of Beck, Fichte, and Schelling, he never completely abandoned his view of the discursivity of the human intelligence, as a protest against which that constructivism had developed. A discursive intelligence could not know the detail of experience *a priori*. But Kant did think it possible to show the rationality of physics to a considerably greater extent than he had once supposed.

5. For all the element of moderation just mentioned, pretty well everybody agrees nowadays that Kant was occupied with a hopeless task in the *Opus Postumum;* and there are many who would judge his performance in M.A.d.N. only one degree less fatuous. The latter opinion, however, is by no means obviously right. All organised inquiries, as Kant saw clearly enough and as others have recognised, rest on *a priori* presuppositions, and one of the tasks of the philosopher is to set these presuppositions out. So far as he attempted to do this for Newtonian physics in M.A.d.N. Kant was occupied with a perfectly legitimate undertaking. Nor is it plain that he was wrong in thinking that a full list of the presuppositions of physics could be arrived at by use of the table of categories. If there are, as Kant thought there were, any ultimate presuppositions of experience as such, it is not unreasonable to suppose that these

will be reflected in the particular presuppositions made by inquirers into different aspects or parts of experience. Kant perhaps exaggerated the value of his famous " clue ", but he was not wrong in thinking that it had importance. Where he was wrong—and this was to have a disastrous effect on his later work—was in describing the principles set forth in M.A.d.N. as constituting " pure " physics and in not recognising their fundamental difference from the other propositions which went to make up that " science ". His failure to see the quite different character of the empirical and *a priori* parts of the discipline led to the mistaken attempt to execute a transition from the second to the first. It may be mentioned that the temptation to this mistake was not confined to Kant's dotage : in the first edition of the *Critique of Pure Reason* he had spoken of the categories as the basis of the highest of all the laws of nature, and of empirical laws as " particular determinations " of these (A 126) : as if he believed that there was no difference of principle between empirical and *a priori* laws, and that the former could, granted time and patience, be deduced from the latter. But to have held this (which he certainly did not hold at the time) would have been, in fact, to abandon the distinction on which Kant built throughout his " critical " philosophy, between what the mind contributes to experience and what it learns from it. So long as he maintained that sensibility and understanding were different faculties, with parts in knowledge which were complementary but did not overlap, so long in fact as he stood by his view of the discursive nature of the human intelligence, Kant could not reasonably maintain that the *a priori* principles of the understanding, either in their general or in their specialised form, could stand on the same level as particular laws of nature. It was perhaps unfortunate, in view of this, that he thought of the results of M.A.d.N. as constituting part of physics at all, despite their apparently physical character. The name " metaphysics of corporeal nature " brought out their distinctive nature much more effectively than " pure physics ".

6. If the results of this discussion are accepted, the consequence is that a sharp distinction is drawn between those physical principles which state the presuppositions of physics and are therefore really metaphysical, and the general body of physical laws ; and the question arises whether Kant would not have been obliged to pronounce the latter unscientific by his own criteria. To this I think the only answer is " yes " ; but it is not clear that those criteria must be accepted as defining all scientific knowledge. And in this connexion it is interesting to note that Kant's own writings contain the materials for an alternative theory of scientific cognition. I refer to the elaborate account of " objective " knowledge which forms the centre of the Analytic in the first *Critique*. The objective, as Kant there explains, is what is related to the objective unity of apperception. The objective unity of apperception, which is the same in each one of us (it is equivalent to the " consciousness in general "

of the *Prolegomena*), is, for reasons which cannot be repeated here, a unity which manifests itself in synthetising, and that is why Kant calls it synthetic. His doctrine, as is well known, is that our understanding subjects all the data the senses present to it to certain *a priori* principles and thus synthetises them into an objective world. The principles in question are necessary principles, not in the sense that they are expressible in analytic propositions, nor yet because they are, as Hume thought, psychologically necessary, but because they are what beings of our special type of discursive intelligence must think. Their necessity, in a word, is "transcendental". Whatever falls under these principles is objective and, in a sense, necessary, for it is thus related to the objective unity of apperception and consequently has a claim to the allegiance of all beings of our type of intelligence. Now it is the business of the "sciences" to describe, and obtain knowledge of, the objective world. If we demand that this knowledge must in every case have apodeictic certainty, in the sense in which we ascribe such certainty to analytic propositions or (on Kant's view) to the propositions of metaphysics, we must deny that empirical "sciences" are sciences at all; for, quite apart from their incomplete character, their results are always corrigible in the light of further experience. But if we attach importance to the fact that scientific statements, whilst not commanding our absolute allegiance, must at the least be such as, in the existing state of our information, have a claim on the assent of all beings like ourselves, then we have an adequate ground in Kant's theory for science of that kind. The laws which we discover in empirical inquiry all fall under the necessary principles of the understanding and are thus related to the objective unity of apperception; and this, as we saw, is the same in each one of us. Because of this there can be objective truths which are not scientific in Kant's strict sense, but which would yet be described as scientific in a perfectly proper sense of the term. It is true, too, that these truths can never form a completed system, for we are always adding to and revising them in the light of further experience; but this does not prevent our systematising them so far as we can. They are far from being the chaotic aggregate of diverse cognitions with which Kant sometimes contrasts scientific knowledge proper.

7. Kant himself would probably have recognised that empirical physics can claim to be scientific in the sense of affording objective knowledge (in M.A.d.N. he speaks of "science in the improper sense" and again of "empirical certainty"); and he would perhaps allow other studies to make similar claims if he were alive to-day. His fault lies not in failing to see the true character of empirical knowledge, but rather in setting up a single scientific ideal and measuring all pretended sciences by it. The consequences of this we have seen: he was forced into the view that the only true sciences were metaphysics and formal logic—he could not allow even mathematics to be properly a science because of its necessarily incomplete

character, despite his ascription of apodeictic certainty to mathematical propositions; and he was led into an unfortunate attempt to rationalise a fundamental part of physics in an unwarrantable way by deducing its principal laws *a priori*, being encouraged here by his own failure to draw a sufficiently clear distinction between the presuppositions of physics, which he tried to set out in M.A.d.N., and the general body of physical laws. He could have avoided all this had he recognised explicitly what his own theories required, that the name "science" can be applied to the results of empirical inquiry if it is taken in a sense widely different from that attached to it in the traditional rationalist account. He should have seen that there are (at least) two types of science, each deserving the name. And it is partly because it leads to this conclusion that the study of Kant's views on these matters is not altogether without philosophical value.

W. H. WALSH.

FACT AND ENTAILMENT.

In his article entitled *The Nature of Entailment*, which appeared in Mind, July 1940, Mr. Malcolm has supported his contention with such clarity that it is a comparatively easy matter to detect any errors in the argument. It seems to me very obvious that an error does occur, an error, strangely enough, of ambiguity. It is not, however, an ambiguity about the *use* of a word, such as that dealt with at such length by Mr. Malcolm himself in the case of the word 'meaning', but an ambiguity about the different *sorts of object* that a word may denote.

I am referring to the word 'fact'. The meaning of this word is not dwelt upon at all by Mr. Malcolm, although the phrases 'one fact' (pp. 344-345) and 'different facts' (p. 345) occur so prominently in his conclusion, which is:

"... is it not obvious that the two sentences do not state different facts ... that there are not two, separate facts, inexplicably connected, but that there is *one* fact differently expressed?"—this being subsequently extended to all cases of so-called entailment.

Fortunately, the word 'fact' is not difficult to define for our purposes. The definition is that 'p' states a fact, or is a statement[1] of fact, if 'p' is true. This involves that there are two sorts of facts according as 'p' is a first or second order statement, and this is in accordance with common usage. Since it is conventionally correct to speak, *e.g.*, about the fact that 'p' entails 'q', or to say that 'if "p" then "q"' is a statement of fact if 'if "p" then "q"' is true.

I shall have to make use of the phrase 'factual connexion', which will perhaps seem question-begging from Mr. Malcolm's point of view. However, I shall make clear from Mr. Malcolm's own examples what I mean by this. It is necessary to use this phrase because, after all, entailment is (as I shall try to prove) a sort of connexion often between different facts, and has therefore to be compared with what Mr. Malcolm has substituted for it (*viz.* with 'factual connexion') if its true nature is to be appreciated. The phrase will serve as a convenient way of referring to Mr. Malcolm's interpretation of entailment, when it is assumed, either for the sake of argument or because it is seen to be the case, that there are two different facts connected by the relation of entailment.

[1] I prefer the word 'statement' to 'sentence', since there is a verb corresponding to the former. To 'sentence' a fact is not to say that a certain sentence is true, but if anything the reverse!

By ' factual connexion ' I mean that connexion of two statements in virtue of which the two apparently different facts stated by them are really expressions of, or expressions of aspects of, one and the same fact. ' Jones has a horse ' and ' Jones has a mare ' are factually connected in this sense, as also are the two sentences, ' the couch is purple ' and ' the couch is not green '. Another example not given by Mr. Malcolm would be that of particular conversion, *e.g.* the two sentences, ' some humans are males ' and ' some males are humans ', which are alternative expressions of the one fact that certain individuals belong to both of two classes of individuals. Mr. Malcolm's contention is that one sentence entails another only in virtue of a factual connexion between them, and therefore that a factual connexion exists in *all* so-called cases of entailment. I shall now attempt to show that the entailment of one sentence by another is *not* in virtue of a factual connexion, and that a factual connexion exists only in *some* cases of entailment. Also, if my argument is correct, I shall incidentally show that factual connexion is one possible ground for entailment, and in some circumstances may be treated as an instance of it : so that we could conclude entailment from factual connexion but not *vice-versa*.

First, I shall trace the process by which, as I think, Mr. Malcolm came to make his mistake. On pages 344-345 he considers cases of pairs of sentences which obviously are factually connected. From this it appears plausible to maintain as an hypothesis that entailment is really nothing but factual connexion. With this tentative conclusion in mind, coupled with a suitable selection of words, it became easy to believe that the example given on page 346 was also one of factual connexion : hence, as the former generalisation now seemed to be true in all cases, it was natural to conclude that there was really no " inexplicable connexion " such as is commonly suggested by the word ' entailment ', but that the so-called process of entailing took place only in virtue of a factual connexion.

Consider this last example, given on page 346 :

" . . . to say that one could know that the table is brown without knowing that if anyone were to believe that the table is brown he would be right, is to say *no more* than that one could know that the table is brown and yet not understand the word ' believe '." This statement seems to me simply incorrect. The condition for knowing ' if anyone were to believe that the table were brown he would be right ' (q) required in addition to the conditions for knowing ' the table is brown ' (p) is not simply the understanding of the *word* ' believe ', if by ' understanding ' is here meant merely a two-term relation between a person and the word. To know ' q ' it is not sufficient to know ' p ' and to be able to use the word ' believe ' correctly. One could do the latter simply by learning a set of rules for the use of the word without in the least understanding the *relation* of believing, and in that case ' q ' would obviously not be known. It is the understanding of this relation, *viz. what is denoted*

by the word, that is the essential condition, in addition to the conditions for knowing ' p ', for knowing ' q ' as well. The attempt to reduce the fact stated by ' q ' to the fact stated by ' p ' therefore fails in this instance, since the fact stated by ' q ' contains something in addition to the fact stated by ' p ', though ' p ' entails ' q '. It could also plausibly be objected, How could the fact stated by the *second* order statement ' q ' be the same fact as that stated by the *first* order statement ' p ' ?

Now consider the general case, in which a statement ' p ' is said to entail a statement ' q '. In virtue of what sort of connexion do we pass from the knowledge that ' p ' states a fact to the knowledge that ' q ' states a fact ? Mr. Malcolm alleges that it is in virtue of the fact that these two apparently different facts are really only one fact of which ' p ' and ' q ' are alternative statements. Can we *state* this one fact ?

(1) Does ' p ' state this fact ?

But the entailment would still hold even if ' p ' stated no fact at all, but were untrue. *For ' if " p " then " q " ', though itself a fact, is hypothetical relative to the fact stated by ' p '.* The fact stated by ' p ' is therefore not the fact we are looking for, since it is not the fact in virtue of which the one statement entails the other, its existence being irrelevant to the entailment.

(2) Similarly ' q ' cannot be a statement of this one fact.

(3) Does ' if " p " then " q " ' state this one fact ?

Though ' if " p " then " q " ' certainly states a fact, this is not the fact that we are looking for, since it is not the fact of which ' p ' and ' q ' are alternative statements ; for as in (1), the fact stated by ' p ' or that stated by ' q ' need not exist, even if the fact stated by ' if " p " then " q " ' exists. Presumably, when Mr. Malcolm speaks of ' p ' and ' q ' as ' expressing ' one fact he means the same as ' stating ' it ? This at least is suggested by the quotation we have already made from page 345, where he says : " . . . the two sentences do not *state* different facts . . ." (italics mine.) If he does not mean the same I do not understand how he is using the word ' express '. The sense would certainly seem to be such as to make ' the *one* fact ' quite as mysterious an entity as the inexplicable connexion ' entailment '. I invite Mr. Malcolm to assist me in the search for this elusive ' one fact ', and to produce *a statement* of it.

Perhaps the attempt to explain entailment in terms of fact would not have been made had logicians followed Russell's advice, that they should never concern themselves with the facts, nor with the question whether what they are saying is true. If two statements *are* statements of the same fact it certainly follows that the one entails the other, but this is only a special case of the entailment of one statement by another. That it is only a special case can at once be seen from the fact that the converse is not true, for if one statement entails another it does not follow that either of them states a fact, so it would not follow that they state the same fact. Entailment

may *be a consequence* of factual connexion, but it *is not* factual connexion, and may occur without it. It is thought to be the same only through a confusion of the facts stated by the statements 'p' and 'if "p" then "q"', which are statements of different order, the first of which is *hypothetical relative to the second*. Therefore the entailment indicates no more than the presence of a formal property common to the two statements, but indicates the presence of no fact beyond the fact of entailment, though it is certainly *possible* that a further fact should exist as the ground of the entailment.

From this my conclusions follow, namely, that the entailment of one sentence by another is not *in virtue of* a single fact of which the sentences are expressions, though if there is such a fact it will involve the entailment of the two sentences, so that such a fact exists in *some* cases of entailment. The fact is a possible *ground* for entailment, and can for some purposes be treated as an instance of it.

E. TOMS.

V.—CRITICAL NOTICES.

The Philosophy of Physical Science. By Sir ARTHUR EDDINGTON. (Tarner Lectures, 1938.) Pp. ix + 230. Cambridge: at the University Press, 1939. 8s. 6d.

PROFESSOR EDDINGTON performs his double function of theoretical physicist and philosopher of science by alternating his mathematical treatises with works directed to a general and philosophical public. *The Philosophy of Physical Science* is related to and based upon *Relativity Theory of Protons and Electrons* (1936) in a similar way to that in which *The Nature of the Physical World* (1928) was related to *The Mathematical Theory of Relativity* (1923).[1] This method of writing has the advantage that the philosophical reader can be confident that Eddington's philosophy has behind it important researches in physics, but it has the serious disadvantage that the philosophical books are not by themselves convincing nor indeed wholly intelligible, and that considerable study of the previous mathematical works is required to elucidate the argument. The difficulty is greater in the case of the book under review (*P.P.S.*) than it was for *N.P.W.*, since the physical theories upon which the latter book was based were, with few exceptions, generally accepted by physicists. But the physics which forms the background of *P.P.S* is a theory of the widest generality developed by Eddington himself in papers since 1928 and expounded by him in *P.E.*; and this general theory has not been accepted by most of the best contemporary physicists. The title is therefore somewhat of a misnomer: "The Philosophy of Physical Science" contains Eddington's philosophy based upon Eddington's physical science. If, as he says in his Preface, "it is the philosophy to which those who follow the accepted practice of science stand committed by their *practice*" (p. vii), neither such people nor most writers on the philosophy of science seem aware of it. In one place Eddington defines physical knowledge as "that which a right-thinking person would to-day accept as justified by physical science", and adds the frank footnote: "'Right-thinking person' is, of course, a modest way of referring to oneself" (p. 2); and in several places (*e.g.* pp. 53, 56) he admits that most physicists would not go the whole way with him in his

[1] The first three of these books, and *New Pathways in Science* (1935), will be referred to as *P.P.S.*, *P.E.*, *N.P.W.*, and *N.P.S.* respectively. All the italics in my quotations are Eddington's own.

philosophy of physics. Nevertheless Eddington frequently speaks as if there were an "ascertainable present-day philosophy of physical science" (p. vii), which he is merely expounding; so the reader should be warned that not only is Eddington's main thesis unacceptable to most of the people whose judgement is of value, but a good deal of the detailed work in mathematical physics upon which he bases his thesis is highly heterodox and does not form part of the agreed physics of to-day. The present reviewer is not in a position to judge whether Eddington's heresies will not be the corner-stone of the physics of the future; but he is bound to caution an unsuspecting reader.

The central proposition in Eddington's philosophy of physics is an original one which, if it were true, would be of revolutionary importance—namely, that the laws of physics are not empirical but *a priori*. "I believe that the whole system of fundamental hypotheses can be replaced by epistemological principles. Or, to put it equivalently, all the laws of nature that are usually classed as fundamental can be foreseen wholly from epistemological considerations. They correspond to *a priori* knowledge, and are therefore *wholly subjective*" (p. 57). Though "it is inadvisable to try to describe a scientifically grounded philosophy by the labels of the older philosophical systems . . . if it were necessary to choose a leader from among the older philosophers, there can be no doubt that our choice would be Kant. . . . Kant anticipated to a remarkable extent the ideas to which we are now being impelled by the modern developments of physics" (p. 189). Eddington's language and emphasis on epistemology throughout the book make it clear that his contention is that the fundamental laws of physics are *a priori* in Kant's sense, and that the reason for their apriority is essentially Kant's reason for the apriority of his synthetic *a priori* judgements—that they are presupposed in the way in which we make judgements. They are "a consequence of the conceptual frame of thought into which our observational knowledge is forced by our method of formulating it" (p. 104). Eddington compares them with the laws of arithmetic (p. 45): just as these (many philosophers have held) are implicit in the process of counting, so the laws of physics are implicit in the processes of physical observation and measurement, and can be disentangled by epistemological and logical study. Eddington's apriorism is not intended to magnify the importance of physical laws: on the contrary, he considers that he is "debunking" them (p. 178) and putting physics in its proper place whence it cannot invade the rightful domains of "life, consciousness, spirit" (p. 69).

Eddington, of course, is not so absurd as to maintain that the particular propositions stating the results of physical observations are *a priori*; about these he is as empirically "verificationist" as any Logical Positivist (pp. 9, 189): it is the "fundamental laws of physics", the "laws of nature", the "regularities which we have

found in our observational knowledge" (p. 67), that are to be *a priori* and subjective. Moreover, "physical knowledge [*i.e.* the more or less general propositions recorded in treatises on physics (p. 2)] comprises, besides 'laws of nature', a vast amount of special information about the particular objects surrounding us" (p. 104); and this special information is not purely subjective. Examples of such information are "that most of space is nearly empty, the matter being aggregated in relatively small islands" (p. 218), and the identity of light with electromagnetic waves, which however, "cannot be treated as an internal hypothesis of physics, since it is altogether outside the province of physics to consider how the stimulation of the optic nerve by electromagnetic waves awakens in consciousness the sensation called light" (p. 44). Nevertheless all the known laws of physics are *a priori* (with the exception of the "law of chance", which stands in a peculiar position which will be discussed later). And this *a priori* physical knowledge includes the determination of the values of the numerical constants which occur in physics, *e.g.* the number of electrons in the universe (136.2^{256}), the ratio of the masses of the proton and of the electron (1847·6), and even the number of dimensions of space-time (4) and of space (3). The deduction of these numbers by pure thought was, for Eddington, the touchstone of his theory. Moreover Eddington is pretty confident that if any "objective" (*i.e.* not *a priori*) laws are discovered, "the result will be very different from present-day physics, and there is no particular reason to expect that it will be called physics. . . . The 'enlarged' physics which is to include the objective as well as the subjective is just *science;* and the objective . . . is to be found in the non-physical part of science" (p. 68). Thus Eddington would actually wish to define the laws of physics to be the *a priori* truths presupposed in our methods of observation and measurement.

I introduced Eddington's thesis by saying that, according to him, the laws of physics are *a priori* instead of being empirical. Here I was using "empirical proposition", "*a priori* proposition" in the exclusively alternative senses that the proposition is one to the truth or falsity of which experience is relevant or irrelevant respectively. Eddington's *a priori* knowledge is certainly not empirical in this sense: it is "knowledge which we have of the physical universe prior to actual observation of it" (p. 24). But Eddington does not describe it as non-empirical; and, like most philosophers, he is vague and (I think) worried as to its exact relationship with experience. Here there are two questions which must be distinguished. First: Are the terms or concepts contained in the *a priori* propositions in some sense empirical? Eddington quite properly answers yes. "A person without observational experience at all, and without that indirect knowledge of observational experience which he might gain by communication with his fellows, could not possibly attach meaning to the terms in which epistemological knowledge, like other physical knowledge, is expressed" (p. 24). In

conventional philosophical language, though the laws are non-empirical, the concepts occurring in them are empirical. Electron is an empirical concept for Eddington just as the number 2 is for Frege and Russell; this does not prevent the law that the number of electrons in the universe is 136.2^{256} being as non-empirical as the law that $2 + 2 = 4$. But to the second question: Is the *a priori* knowledge itself sometimes or always in some sense derived from experience? Eddington gives unsatisfactory and doubtfully consistent answers. I have quoted his statement that it is "prior to actual observation" (p. 24), which sounds definite enough. But he frequently speaks of such knowledge as being also obtainable "*a posteriori*" "by study of the results of observation" (*e.g.* pp. 24, 104). How can knowledge of laws which are not based on empirical facts be derived from observation of empirical facts? The difficulty is one which W. E. Johnson tried unsuccessfully to explain by his "intuitive induction"; and Eddington cannot be blamed for failing to solve what is for him a minor problem which has baffled philosophers from Locke's time onwards. If Eddington's position is described in the following language, I imagine he would agree: "The fundamental laws of physics are *a priori* and the reasons for accepting them are never empirical observations. Most people, however, can only come to accept them as true at a time when they are also thinking of genuinely empirical propositions stating the results of physical observations. It is easy, then, to be confused as to the rôle of these empirical propositions; to think, mistakenly, that they are reasons for accepting the laws; and thus to think that the laws themselves are derived *a posteriori*. Nearly everyone except myself makes this mistake."

Whatever the sense in which the laws of physics are derivable *a posteriori* from experience, it is quite certain that Eddington believes that they can be demonstrated *a priori* from epistemological considerations alone (*e.g.* pp. 25, 104). His epistemological deduction is thus the most important feature in his present philosophy of physics, and upon its validity the whole structure stands or falls. Unfortunately in this book (*P.P.S.*) he gives indications only of how the deduction is to be performed, and refers readers to *P.E.* for further details. But even there, in the Introduction, he writes: "I think it will be found that the theory is purely deductive, being based on epistemological principles and not on physical hypotheses. But it could not be presented in purely deductive form—which would mean, I suppose, that it was treated as an investigation in pure mathematics with a physical *dénouement* in the last chapter" (*P.E.*, p. 5). Though presumably an epistemological deduction would have important differences from a formal mathematical one, yet it is surely self-contradictory to say both that a theory is "purely deductive" and that it "could not be presented in purely deductive form". Certainly Eddington nowhere attempts to do so; and this makes it exceedingly difficult to examine the validity of his argument.

The effect produced upon me by a study of Eddington's three last books (*N.P.S.*, *P.E.*, *P.P.S.*) is similar to that produced by reading reasonable defences of theism by philosophers of repute (*e.g.* many series of Gifford Lectures). It is impossible to deny that some weight should properly be attached to the various theistic arguments (*e.g.* the cosmological argument or the argument from design); but the degree of this weight depends very largely upon one's previous attitude to the theistic hypothesis. Eddington's arguments are such that, if one was nearly converted beforehand to his thesis, the conversion might be completed: his evidence is all of the nature of circumstantial evidence, which supports an already strong case but is otherwise capable of being explained away.

I should, however, be shirking my duty if I did not examine in some detail one example of Eddington's reasoning which is vital to his central thesis. If his argument were presented in purely deductive form, this example would appear in the first chapter: in Eddington's presentation, it occurs on pp. 162-164 of *P.P.S.* and in scattered sentences in the last chapter of *P.E.* Because it involves no considerations of physics, it has not (to my knowledge) been discussed by scientific reviewers; and since, besides one mathematico-logical mistake, there is also an error in the application of mathematics, *i.e.* in what Eddington calls "epistemology", it is perhaps appropriate to treat it, even at some length, in the pages of MIND.

The mathematical foundation of Eddington's physics is a linear algebra (a set of hypercomplex numbers) based upon sixteen distinct square roots of -1. This algebra of E-numbers (as Eddington calls it in *N.P.S.* and *P.E.*) is mathematically equivalent to the direct product of two quaternion algebras, to the algebra of fourfold square matrices, and to the algebra of the sixteen distinct rotations in a six-dimensional space (which is how it is described in *P.P.S.*).[1] It is non-commutative with respect to multiplication, *i.e.* it is not always the case that $ab = ba$. This non-commutativeness is quite essential to its use in physics, for quantum theory has required a non-commutative mathematics since Heisenberg's famous paper of 1925. From the properties of this algebra Eddington obtains (by arguments whose weight it is difficult to estimate) the fundamental laws and the constants of physics. In particular he derives the structure of space-time and its division into three dimensions of space and one of time from the property that the basic elements fall into "anticommuting pentads" which are such that, for any pair of terms a, b of the five, $ab = -ba$. Thus this non-commutative algebra plays the leading role in Eddington's system; and a critical point in establishing his thesis is the deduction of this algebra from epistemological considerations, *i.e* the proof that this abstract algebra rather than the hundreds of others that mathematicians

[1] Eddington gives a simple example of the E-algebra in *N.P.S.*, pp. 267-271. It is presented in the mathematically purest form by J. E. Littlewood, *Journ. Lond. Math. Soc.*, 9 (1934), pp. 41-50.

can invent is the algebra that must be applied to the physical world.

The argument runs as follows: (A) "Observation can only disclose the relations between entities; and the most elementary relation we can consider is the relation between two elementary particles. . . . This relation only exists if both the particles exist" (*P.P.S.*, p. 163). (B) "In mathematical language [existence is represented by] a symbol J with two eigenvalues, which are most conveniently taken to be 1, standing for existence, and 0, standing for non-existence. The symbol J must satisfy the equation $J^2 - J = 0$, since that is a quadratic equation which has just the two solutions $J = 1$ and $J = 0$. Another way of writing the same equation is $J^2 = J$. We call a symbol which is equal to its own square an *idempotent* symbol. *The structural concept of existence is represented by an idempotent symbol*" (p. 162). (C) "The relation between two elementary particles with simple existence symbols J_1 and J_2 respectively" is to be assigned "the double existence symbol $J_1 . J_2$, which will have the existence eigenvalue 1 if both J_1 and J_2 have the eigenvalue 1, and the non-existence eigenvalue 0 if either or both have the eigenvalue 0" (p. 163). (D) The algebra of these double existence symbols is the algebra of E-numbers (p. 164). "Since J has two eigenvalues, it can be represented as a twofold matrix. Then $J_1 . J_2$ is represented by a fourfold matrix, or equivalently by an E-number" (*P.E.*, p. 326). That the physical world obeys the rules of the E-algebra is thus due to this algebra being that of double existence symbols.

I believe that (D) contains a straight mathematical mistake, and that the E-algebra cannot be got out of the "existence symbols", whose algebra is quite a different one. The laws of the J-algebra given by Eddington are the three already quoted, which he says are equivalent—(1) Every J has two eigenvalues 0 and 1, (2) $J^2 - J = 0$, (3) $J^2 = J$, together with (4) $J_r J_s$ is itself a J (implicit in *P.P.S.*, p. 163), and (5) $J_r J_s = J_s J_r$ (*P.E.*, pp. 308, 325). He makes no explicit reference to laws of addition of the Js; but since he takes (2) and (3) to be equivalent he must be implicitly assuming (6) $J_r + J_s$ is a J, (7) $J + 0 = J$, (8) $J + (-J) = 0$. Adding to these the laws that addition is (9) associative, (10) commutative, that multiplication is (11) associative, (12) distributive with respect to addition, and (13) that $J . 1 = J$, all of which I am sure Eddington would grant (many of them are implied by the stated equivalence of (1) and (2)), the system of the Js has a well-known algebra—the algebra of an idempotent ring having a unity element.[1] And if (1) be taken to mean that every J is either 0 or 1, the ring has only two elements. Now the algebra of idempotent

[1] Propositions (4)-(12) determine that the Js form a ring, (3) that the ring is idempotent. (5) is a consequence of (3), (4), (6)-(12): so, though Eddington does not assert (5) explicitly in *P.P.S.* (as he does in *P.E.*), it is implicit in *P.P.S.*, pp. 162-163, that the multiplication operation is commutative.

rings is a thorough-going commutative algebra, and it is impossible for Eddington's non-commutative *E*-algebra to form a part of it. Nor can the *E*-algebra be an extension of the *J*-algebra in the sense in which the algebra of complex numbers is an extension of that of real numbers. In the real number algebra an operation is defined—taking a square root—which is such that it is not the case that the square root of every real number is itself a real number. The algebra of real numbers is thus not closed with respect to all its operations. It is therefore natural to include it in a wider system—the algebra of complex numbers—which is so closed. But the algebra of idempotent rings is closed with respect to all its operations; so there is no escape for Eddington that way. In the one place where he appears to recognize that he is not using but extending the *J*-algebra, he justifies the extension by saying that to refuse to do so "would be contrary to the ordinary conceptions of transformation theory" (*P.E.*, p. 325 n.). But transformation theory is a development of ordinary numerical mathematics, so there seems no reason why a non-numerical algebra like the *J*-algebra should not run contrary to it.

The truth is that Eddington, in spite of all that he says about getting all the mathematics he wants out of the idempotency of the *J* symbols, employs them in accordance with the laws of ordinary algebra whenever he thinks fit. This is shown by his whole treatment of them in *P.E.* (*e.g.* his representing them by numerical matrices); and in *P.P.S.* by his use of the technical word "eigenvalue", which is only defined in a system which includes numbers, and of expressions like $2J - 1$ (p. 165), which, if the *J*s obeyed the laws I have stated and $2J$ is defined as $J + J$, would always equal 1. $[J + J - 1 = J + J + 1 = 0 + 1 = 1.]$

I have criticised Eddington's reasoning on abstract grounds, since that is how he presents it. Its unplausibility can be shown more simply. If we interpret his *J* symbols as standing for propositions—$J_a = 1$ or 0 as meaning that the proposition "*a* exists" is true or false respectively—and the algebra as the propositional calculus[1] (which is the natural interpretation of Eddington's language), his contention would be that the abstract geometry of six-dimensional space is deducible not from its own axioms but merely from the primitive propositions of the propositional calculus and of arithmetic. If this were the case, it is incredible that it should have escaped the notice of Peano, Whitehead, Russell, Hilbert.

[1] The algebra of an idempotent ring with a unity element is identical with the Boolean algebra, and that of an idempotent ring with only two elements 1 and 0 is identical with the two-valued Boolean algebra, *i.e.* the propositional calculus, if the multiplication and addition operations of the ring be taken to be the Boolean "and" and the Boolean *exclusive* "or" respectively. This was proved by Orrin Frink and M. H. Stone. For a general account of recent work on Boolean algebras and idempotent rings see M. H. Stone, *Bull. Amer. Math. Soc.*, 44 (1938), pp. 807-816.

In order to discuss the epistemological stages in Eddington's argument—(A) and (C), it is necessary to refer to the importance he attaches to the concept of structure. For Eddington physical knowledge (which includes empirical knowledge of special physical facts as well as *a priori* knowledge of laws) is purely structural knowledge (pp. 150, 216): unlike "the vivid knowledge which I have of my own sensations and emotions" (p. 142), this knowledge of structure is communicable, and can thus provide us with a public body of knowledge. This dichotomy of knowledge—private, incommunicable *Erkenntnis* of the "content" of experience on the one hand as opposed to public, communicable *Wissenschaft* of "form" or "structure" on the other—has been prominent in the writings of many recent philosophers (*e.g.* Russell's *Analysis of Matter* and Schlick's London lectures published in his *Gesammelte Aufsätze*). I believe that the dichotomy is invalid; but since Eddington has nothing new to say upon the general question and since he never discusses in detail the problems of perception, I shall not argue the matter here.

Unlike many writers who use the concept, Eddington does define "structure", and his definition is relevant to my discussion. "To be quite precise, it is structure of the kind defined and investigated in the mathematical theory of groups" (p. 147). And group theory, according to Eddington, is the theory of a "terminable set of operations" *P*, *Q*, *R*, etc., which are such that the operation of changing *P* into *Q* is not a new operation but is itself an operation of the set (pp. 138-140). Here Eddington by his use of the deceptive word "changing" conceals an essential feature of the theory of groups—namely, the set of elements of a group (call them operations if you like) do not form a group in themselves, but form a group *with respect to* a given mode of combination. The mathematical definition of "group" is that *P*, *Q*, *R*, etc., form a group with respect to a mode of combination *C* if (1) the result of combining any two members of the set by means of *C* is itself a member of the set (*i.e.* the set is closed with respect to *C*), (2) combination by means of *C* is associative, (3) for any two members of the set, there is a third member of the set such that the result of combining the first and the third member (or the third and the first) by means of *C* is the second member. (See *e.g.* Albert, *Modern Higher Algebra*, p. 7.) All three parts of this definition refer to *C*: a group, in the mathematical sense, is undefined except when the combining relation is given. *P* is not changed into *Q* by operating on it with another operation *R* of the group (as Eddington says), but by operating on it by combining it by means of *C* with *R*—quite a different thing. When Eddington gives the set of rotations as an example of a group (pp. 139, 145), he is implicitly assuming that the combining of one rotation with another is effected by performing it immediately after the other. If the mode of combination were instead taken to be, *e.g.*, that of expressing the rotations by symbols on the same black-

board, the result of such a combination would not be a rotation, and rotations would not form a group with respect to that mode of combination. The group-structure which Eddington is talking about must be less abstract than his language leads one to suppose. To say that two sets of things have the same group-structure would be to say nothing of interest unless the modes of combination of both the groups had been specified.[1] The fact that structure depends upon content is one reason why the structure-content dichotomy of knowledge is untenable.

To return to Eddington's argument on pp. 162-164. He states (A)—that " observation can only disclose the relations between entities "—without giving any reasons for it, nor for the similar dogmatic statements in *P.E.* (pp. 3, 323). I suspect that the grounds on which he accepts it are threefold :—(1) The only form of observation yielding communicable knowledge is observation of group-structure ; (2) The only form of observation relevant to physics is observation of spatio-temporal relationships (*cf.* his emphasis on " pointer readings ", *N.P.W.*, p. 251, *P.P.S.*, p. 99) ; (3) " For the purposes of physics the only value of the direct awareness described as I-perceive-the-sound-of-the-Greenwich-time-signal is that it can be compared with and in some cases recognised to be the same as another direct awareness which I remember to have had " (p. 207). These grounds do not seem to me adequate nor mutually consistent, but I will not discuss them, since I am prepared to grant a modified form of (A) which is sufficient for Eddington's purpose—namely, that observation of relationships (I think spatio-temporal relationships between events) is presupposed in physical knowledge. [I am not prepared to grant Eddington's further contention that measurement presupposes tetradic relations (p. 168), since I believe that measurement is got out of comparisons of two extended events, not out of comparisons of " displacement " relationships each between two entities.] But, granted a modified (A), it is (C) which I find incredible. In defence of it Eddington writes : " We want to express symbolically the fact that *P*, *Q*, *R*, are *relations*. The structural concept of a relation is that it is something whose existence is contingent on the existence of two entities either of which could exist or not exist. Thus, in representing *P*, *Q*, *R* by double existence symbols, we are saying no more about them than is necessary in order that they may be conceived as relations " (p 164). But are we saying enough to represent their " structural concepts " ? " A structural concept is obtained from a corresponding general concept by eliminating from our conception everything which is not

[1] For an elaboration of this point see M. H. A. Newman, MIND, N.S., 37 (1928), pp. 137-148. This article is concerned with Russell's use (in *The Analysis of Matter*) of structure in the sense of *Principia Mathematica*, but his strictures are applicable to Eddington's group-structure. If Newman's conclusive criticism had received proper attention from philosophers, less nonsense would have been written during the last twelve years on the epistemological virtue of pure structure.

essential to the part it plays in a group-structure" (p. 144). As we have seen, what is essential to this is the mode of combination of the elements; and if we say no more of particular dyadic relations than that they are dyadic, which is what representing them by double existence symbols is doing, we are ignoring exactly that feature of "content" which must be used in a particular case to specify the mode of combination of P with Q. For example, take P and Q to be rotations. The combination of P with Q is then defined as the result of performing the two operations *in succession*, and this definition is used because it is appropriate to P and Q being rotations. If P and Q were purely spatial relations, another mode of combination would be defined appropriately. So the "structural concept" of a relation—the part it plays in a mathematical group—cannot be divorced from the nature of the relation itself; and to represent it by a symbol which expresses merely the existence or non-existence of its relata is, on Eddington's own way of thinking, to misrepresent it.

Moreover, to represent a dyadic relation P by a double existence symbol is, in a different way, to say too much. $J_a \,.\, J_b$ represents, presumably, that the two elementary particles a and b both exist. But to say that a dyadic relation P exists implies that it holds between some two things, not that it holds between the two specific things a and b. Eddington is confusing the relation P with the relationship aPb. The rotations which form a group with respect to the mode of combination of immediate succession are not "rotationships" from one specific point to another. A "rotationship" from London to Louth cannot immediately succeed a "rotationship" from Buckingham to Fishguard, but a rotation of 1·9° north in a meridian plane can succeed a rotation of 4° west in the plane of a parallel of latitude. If Eddington were serious in presupposing existence of the individual elementary particles at the beginning of his chain of proof, it would be circular to attempt to deduce (as he does) the number of elementary particles in the universe.

Eddington's attempted deduction from epistemological principles of the laws of physics thus fails, in my opinion, at the first stage—the proof that the E-algebra is the one and only algebra applicable to the world of physical observation. It seems amazing that Eddington should have attempted this proof in the way he has done. For he first rejects (as not sufficiently abstract) the feature of asymmetry in some of the relations we observe which permits of the application of the mathematical notion of ordered series, and of the whole of ordinary numerical mathematics, from which his non-commutative E-numbers can be obtained in various ways. He then expects to obtain his non-commutative system from the rump of the propositional calculus, which is all that he has left himself. No wonder he fails. A far more hopeful line would surely have been to direct attention to the fact that the E-algebra is the direct product of two quaternion algebras, and to try an epistemological deduction of one of the well-known geometrical applications of quaternions.

But I fear that he would have to assume that space has three dimensions and many geometrical properties, and not be able to deduce these by pure thought.

I must discuss very briefly Eddington's treatment of the " law of chance " or " principle of non-correlation ", and its status in physics. At the beginning of this review I questioned Eddington's right to assume the general approval of physicists for either his physical theories or his philosophy of physics; but I do not dispute his right to speak for contemporary physics when he says: " The entire system of laws of physics at present recognised is concerned with probability. . . . As a means of calculating future probabilities the laws form a completely deterministic system; but as a means of calculating future observational knowledge the system of law is indeterministic " (p. 94). The resistance which many philosophers (including myself) have had against accepting the irreducibly statistical nature of the laws of physics was due partly to a hope (now disappointed) that physicists would be able to derive their " indeterministic " laws from more fundamental " deterministic " ones and that to accept the " law of chance " as ultimate was to admit defeat; partly to an irrational disinclination to regard physical laws in the rather complicated manner required; and partly to an erroneous belief that acceptance of the new " indeterministic " view of physical law would make it less likely that the sun would rise to-morrow. That the fundamental physical laws are statements of probabilities (in the sense of relative frequencies within a class) is a revolutionary change in the conception of physical law; but it is a change within physics itself, and has few philosophical consequences—certainly not the sensational ones that have been attributed to it, by Eddington among others. However, in discussing the free-will problem Eddington now rightly rejects as nonsense a hypothesis he had considered in *N.P.W.*, (p 311)—that volition might determine the exact behaviour of material particles within the range permitted by the statistical physical laws; since " there is no half-way house between random and correlated behaviour " (*P.P.S.*, p. 182). Eddington's defence of free-will is now straightforwardly the good old contention that mind can and does interfere with the laws of physics (p. 219), the only novelty being that the physical law with which it interferes is the " law of chance ". The question of indeterminism which arises in most discussions of free-will is thus properly handed back to the physiologists, biochemists, and biophysicists, who (I believe) would give a different answer from Eddington's.

With regard to the nature of the " law of chance ", which must now (thirteen years after its introduction by Heisenberg) be accepted as an ultimate physical principle, Eddington in one place (p. 98) toys with the idea that it is in some way implicit in the publicity of a physics based upon the experiences of a number of " perfect observers ", and is hence epistemological in origin. But he does not seriously develop this view; and in other places (pp. 61, 181,

218, 221) he treats the "law of chance" as a physical hypothesis which is not *a priori*, and the well-established fact that it holds of "matter unassociated with consciousness" as a "special fact about the world in its present condition". That it does not fall into line with the other fundamental laws of physics is not surprising when its negative character is appreciated: "the law of chance or non-correlation is not one of the fundamental laws of physics but the word 'Finis' added when the list is complete" (p. 220). Whatever may be thought of Eddington's belief that the "law of chance" is not universally valid and that the "special fact" about the world is the rarity rather than the absence of correlation, there can be no doubt that the "law of chance" is a second-order principle stating that the first-order fundamental laws of physics are essentially statistical and do not determine the behaviour of individual particles. In Carnap's language, it is a "syntactical" proposition in the "logic" of physics. Thus it is no criticism of Eddington's apriorism that he does not deduce the "law of chance".

The Philosophy of Physical Science includes interesting discussions of many other topics in physics and philosophy which I am unable to treat in this review. In particular, I have not criticised the purely philosophical parts of the book, because they contain little that is original though much that is sensible and acute [*e.g.* the logical difficulty in "hypothetico-observational" knowledge (p. 13), the role of "sympathetic understanding" in solving the solipsism problem (p. 190), the alleged distinction between sensations and sensa (p. 212)]. Since Eddington now regards himself as investigating primarily the nature of physical knowledge rather than the nature of the physical world, he no longer talks of different contrasting worlds (of scientific tables and of familiar tables) in the way which (in *N.P.W.*) Professor Stebbing and other philosophers have found so confusing. In the present book (*P.P.S.*) it is the substance, and not the mode of expression, of his doctrine that will shock empiricists. Many "epistemological physicists" from Mach onwards have called attention to the fact that sentences in physical treatises which in the past have expressed empirical propositions are now used to express definitions or logical consequences of definitions, and that as physics advances there are changes in the conventions determining which sentences express the *a priori* and which the empirical. But, to my knowledge, Eddington is the first writer to maintain the position, qualitatively and not merely quantitatively different from the proper recognition of the conventional element in modern physics, that every genuinely fundamental law of physics is non-empirical and *a priori*. I cannot close this very critical notice without expressing my admiration for his solitary heroism in defending such a desperate position. What a philosopher regrets is that Eddington has not thought fit to argue his central thesis in the systematic manner it requires. He has yet to give us his *Critique of Physical Reason*.

R. B. Braithwaite.

Spinoza's *Tractatus de Intellectus Emendatione:* A Commentary. By the late HAROLD H. JOACHIM. Oxford, Clarendon Press, 1940. Pp. xvi, 231. 15s. net.

THE last few years of the late Professor Joachim's life, we are told, had been largely devoted to this work. He had nearly finished it, but was held up at the end by the difficult questions connected with the unfinished state of the *Treatise* itself; and he had also intended to submit the whole to a thorough revision. Sir W. D. Ross, whose advice was asked, recommended that the work should be published as it stands, and he has unquestionably been right to do so, for the work not only deals with Spinoza's views about knowledge, method, error, etc., in greater detail than was appropriate in the author's previous book on the *Ethics*, but also gives Joachim's latest views on some of the fundamental questions in Spinoza's metaphysics.[1]

The opening paragraph of ch. i gives the following account of the object of the book: "to study some of the leading philosophical ideas in Spinoza's unfinished 'Essay on the purification of the intellect'. It will be concerned in the main with Spinoza's teaching in regard to knowledge and its method; in regard to truth, supposal, doubt, and error; and in regard to the nature, powers, and limitations of the knowing mind." The words "to study some of the leading philosophical ideas" are significant. A commentary is, of course, whatever the commentator chooses to make it, but we perhaps usually think of the commentator as primarily concerned with the continuous explanation of the text before him, although he may also refer, if necessary, to other works of the author for illustration and comparison. But Joachim's aim lies beyond this; he wants, in the first place, to connect the ideas of T with the general metaphysical doctrine of Spinoza, especially as contained in the (not then written) E, and, in the second place, to determine how far the ideas so illuminated are philosophically satisfactory. For such a procedure there is this much to be said: that T is not separated by any great interval of time from the earlier *Short Treatise* on the one hand and the commencement of the later E on the other. Thus the general metaphysical doctrine of Spinoza must have been before his mind in some form when he was writing T. Still, Joachim himself says (p. 218 n. 3) "it is far from certain what stage in his philosophical development Spinoza had reached when the Treatise was composed", and he expressly warns us that "the treatment of the human mind in the *TdIe* is conducted, for the most part, on an empirical and popular level—deliberately so conducted, as Spinoza himself explains" (p. 89, with quotation from *Ep.* 37). I am inclined to think, therefore, that it would have been a safer procedure, and fairer to Spinoza, to take T as far as possible by itself in the first instance, and then consider its relation to the general metaphysical doctrine later. But Joachim's readiness to enter into (and

[1] It will be convenient to refer to our *Treatise* as T and to the *Ethics* as E.

even digress into) metaphysical discussions adds greatly to the philosophical interest of the book, which, besides being a commentary on T, becomes also an important supplement to his previous *Study of the Ethics of Spinoza*.

The short introductory ch. i is concerned with preliminary questions about the relation of the Latin text of T to the Dutch version, the time of composition of T, the *opusculum* to which Spinoza referred in a letter to Oldenburg and of which our T was apparently to have been a part, and finally Spinoza's later intentions in regard to the completion or re-writing of T.

Ch. ii first summarises the introductory sections which contrast finite goods with the true good and affirm that our highest good is to know 'the union of our mind with the nature of things as a whole', and that our primary need, therefore, is to find some way to remedy the weakness of our intellect. The larger part of the chapter is then devoted to a discussion of the four ways of knowing. Spinoza, of course, finds that the best way is the fourth, in which we have direct or intuitive knowledge of the essence of a thing. In the discussion of this conclusion Joachim first notes how Spinoza's view of essence differs from the Aristotelian, and then shows the difficulty of applying the Spinozistic reciprocation of essence and existence to finite things. Even if we concede that finite things are unique individuations of the one substance, we only raise the further difficulty how we can have an intuitive knowledge of them in that character. '*Scientia Intuitiva*' cannot be for Spinoza a mere unattainable ideal, yet we cannot see how our knowledge of finite things can attain to it, though Spinoza seems to have thought it did in regard to the eternity of the human mind. If, again, we take his mathematical examples of *scientia intuitiva* as our clue, we must admit their disparity with the case, *e.g.*, of the human individual. This criticism (pp. 37-51) of *scientia intuitiva* is an example of the way in which Joachim passes beyond the text of T into a discussion of ultimate problems in Spinoza's philosophy.

Ch. iii is entitled 'Knowledge and its Method'. It has to deal almost at once with the distinction of idea and ideatum. After stating the account of it which Spinoza gives in §§ 33, 34 Joachim continues as follows (p. 55): "On the one hand, then, a true idea is other than, and 'quite different from', its *ideatum;* and yet, on the other hand, they are inseparably united, and their union is, in a certain sense, identity. The true idea of Peter is of Peter and no other *ideatum*, while Peter is the *ideatum* of this and no other true idea; and it is usual, therefore, to speak of a 'mutual implication' or an 'essential correlation' of Peter and the true idea of Peter. But this is not a strictly accurate description of the situation. There is here no *correlation* (no reciprocal connexion, no mutual implication, of two things or terms), but a *two-sided identity*—a single identical term, or 'something' displaying its *concrete* singleness in a duality of aspects, or showing itself in two differences which inseparably

express its *concrete* identity." There seems to be no occasion or warrant in the text for this statement about a single identical term. Indeed the statement contradicts, or at least obscures, the very point which Spinoza is expressly asserting, *viz.*, that idea and ideatum are two quite distinct entities. And what are we to make of the statement itself? By 'the true idea of Peter', we are told (p. 54 n. 2), "Spinoza here means the true idea which some one else may form, or have, of Peter". But how can Peter and some one else's idea of Peter be regarded as two aspects of a single identical term? Joachim explains that the two-sided identity is the essence. "*On one side of itself*, Peter's essence . . . is his 'formal essence' or what he *really* is . . . *On its other side* . . . Peter's 'essence' is what he is ideally (*objective*), what he is 'in the true idea of him'." But Spinoza does not say here that formal and objective essence are two sides of one thing; he says emphatically that they are two quite different things. And what is meant by saying 'On its other side Peter's essence is what he is ideally'? Peter's essence is as real as Peter is; it is not anything 'ideally'; and the fact that somebody may have a true idea of Peter's essence is surely not an 'other side' of that essence itself. Joachim repeats the kind of phraseology he had used in the *Study* (pp. 147-148). He says (p. 56) "A true idea, then, is both the same as, and other than, its *Ideatum*". We can see better why he used such language (inaccurate though it is) in the *Study*, for he is there interpreting the distinction between idea and ideatum in the light of the doctrine of attributes and of E ii, 7 S, where Spinoza uses the phrase *eadem res duobis modis expressa.* But the statement of the distinction in T is quite general or logical, not metaphysical; and where the ideatum is itself an idea—a case which Spinoza himself uses in explaining the distinction—one attribute only is (directly or relevantly) involved. Joachim himself tells us (p. 90) that "the doctrine that the mind is the *essentia objectiva* (the 'idea') of the body is virtually ignored" in T; yet, since he regards the metaphysical interpretation of the distinction between idea and ideatum as giving Spinoza's 'main teaching in regard to knowledge and truth', he thinks the use of the notion of *idea ideae* in T must involve an inconsistency (see pp. 99 with n. 2, and p. 105). But why should our interpretation and criticism of T be required to conform to the doctrines of E?

After his discussion of the relation of the true idea to its ideatum Joachim says, "Underlying this account of a 'true idea' and our 'possession' of it are two of the leading doctrines of Spinoza's philosophy" (p. 57). These doctrines are (1) that every idea is an act of mind, (2) that we are parts of an *ens cogitans.* But is there any warrant for the word 'underlying'? Surely the distinction between idea and ideatum is quite independent of these two doctrines. The mention here of the second doctrine—Spinoza himself refers to it in a later § (73) and in a different context—leads Joachim to enter upon an Excursus (pp. 60-88) in which he discusses the relation of

finite modes to the attributes. This excursus goes quite beyond the scope of our treatise, but, taken in conjunction with the previous discussion of *Scientia Intuitiva*, it is for the student of Spinoza's general philosophy perhaps the most valuable part of the whole book. For it brings under review in a comparatively short space a number of the ultimate difficulties in Spinoza's system, and shows how these difficulties arise out of Spinoza's own doctrine and where he fails to satisfy us. I must be content with some brief indications of the line of argument. The Excursus begins by contrasting Spinoza's doctrine of substance with that of Descartes, and then discusses the way in which Spinoza conceives the two attributes and refers to the special difficulties which arise in regard to the relation of Thought to the other attributes. Then it deals with the problem of the relation of the finite modes to the attributes and infinite modes. Spinoza does not and could not deduce the finite modes from the infinite modes; on the other hand, to connect finite mode with finite mode leads only to an endless progression. Hence it is difficult to see how we can have any adequate knowledge, whether intuitive or scientific, of the relation of finite to infinite modes. Spinoza has more to say about Extension than about Thought, and in one place he suggests the notion of an " ascending hierarchy of finite bodies " of ever-increasing complexity which would culminate in an " Infinite Corporeal Individual " ; and this notion might seem in a measure to bridge the gap. But he does not even suggest that there are " grades intervening between the human mind and the ' Infinite Idea of God ' ", so we are left wlthout any clue as to the manner in which the former takes its place in the latter. In short, the result of the Excursus seems to be that the doctrine that the human mind is part of an infinite intelligence is one which is much more in need of illumination than capable of affording it.

The next part of the chapter professes to consider Spinoza's view of advance in knowledge, but it is really taken up for the most part with further discussion of the views of Descartes and Spinoza about truth and the adequacy of ideas, and I will pass at once to the remaining part of the chapter, which deals with Spinoza's view of method. In § 38 Spinoza says (1) that method is nothing else than *cognitio reflexiva* or *idea ideae*, and (2) that, since there cannot be an *idea ideae* unless there is first an *idea*, there can be no method unless there is first an *idea*, *i.e.*, as Joachim translates, ' no method unless there is first knowledge on which to reflect '. Now whatever Spinoza means by *idea ideae* here, (2) surely is self-evident.[1] But Joachim proceeds to comment as follows: " It seems to follow that Method can neither guide nor control the knowledge of which it is the Method. For the reflective knowledge, which is Method, presupposes . . . that true ideas have formed and linked themselves together within the

[1] Joachim later (p. 109) concedes, as of course he must, an elementary truth to Spinoza's statement, but thinks that the statement carries a further and erroneous implication.

reflecting mind. And if, in reflecting, the knower were to remodel or rearrange the true ideas on which he reflects, he would be tampering with his knowledge—perverting it into error" (p. 106). And for an explanation of *idea ideae* he refers to the statement in E ii, 21 S that it is the 'forma ideae quatenus haec ut modus cogitandi absque relatione ad objectum consideratur'. Joachim admits that this explanation is 'brief and obscure', but why should he drag it in at all? *Idea ideae* there = *idea mentis*, and why should an obscure statement made in a later book about the *idea mentis* be used in criticism of what is said about reflection upon true ideas here? Spinoza's general statements about method are vague and far from clear, but he does not, so far as I see, propose that the knower should 'remodel the true ideas on which he reflects', but only that the inquirer should take as patterns ideas of whose truth he is certain, and by reflecting on them learn to distinguish true ideas from ideas that are not true, and so on (§ 49). But Joachim thinks that, unless the difficulties which he indicates in the passage quoted above, and those which he infers from the explanation of *idea ideae* in E, can be dispelled or overcome, "Spinoza's whole project in [T] must be condemned as illusory; for we shall have to admit that there neither is, nor can be, a method such as he there describes" (p. 107). The chapter, accordingly, concludes with a 'rough sketch' of the 'reconstruction' which T would 'have to undergo in any attempt to weave its component doctrines—its views of reflexion, knowledge, and method itself—into a coherent theory' (p. 108). It seems to me that in this discussion of method, as in the previous discussion of knowledge, Joachim's determination to connect the simpler logical doctrines of T with the metaphysical doctrines of E, and to do so even where Spinoza himself does not indicate a connexion—interesting and valuable as the result may be from a systematic point of view—interferes seriously with the unprejudiced interpretation of the text of T itself.

Ch. iv is entitled: 'The Three Types of Pseudo-Cognizance: Supposal,[1] Error, and Doubt', *i.e.*, the *idea ficta, falsa, dubia* of Spinoza. The chapter is the longest in the book, perhaps partly because it is concerned with topics some of which are not dealt with in E. (i) Joachim expounds Spinoza's general view about supposals of existence and supposals of essence, and comments on the difficulty, especially in regard to the latter, of understanding what an *idea ficta* is as distinct from an *idea falsa*. This difficulty is met by carrying the exposition a stage further and supplementing it by an account, derived from E, of the working of imagination. But this more detailed account only lands us in a final and unanswerable difficulty. Supposal involves a semblance at least of judgment; the *idea ficta*

[1] 'Supposal' is not a good rendering of Spinoza's *idea ficta*, for what is merely 'supposed' is contrasted with what is true or real, whereas the *idea ficta* may be entertained and acquiesced in. In a technical book the simplest plan would be to retain the Latin term.

is liable to be mistaken for a true idea. But the more we stress the purely passive character of the *idea ficta* as a mere product of the association of ideas, the more the semblance of predication disappears from it. An idea may come into the mind without seeming to be true. (ii) After an exposition of Spinoza's account of the *idea falsa* Joachim first points out the weaknesses of the account. Apparently the *idea falsa* may have the same content as the *idea ficta*, and differs from the latter only in giving assent to this ideal content. Both assertions seem untenable. If there is no difference in the content, why is there assent in the one case and not in the other? And if the assent is an act of the intellect, can the *idea falsa* be opposed to knowledge as if it were a mere negation of the latter? Joachim thinks that there is in T (as a whole) a cleavage between imaginational experience and knowledge which in E (as a whole) is transformed into a difference of degree and value. And this leads him to ask whether even the view taken in E can be reconciled with Spinoza's doctrine of the relation of the human mind to the infinite intellect. The few pages which deal with this question are of great interest and value to the student of Spinoza's general philosophy. The explanation of error in terms of privation is common to T and E, and Joachim concludes this section with an exposure of the difficulties of this doctrine. (iii) T deals rather briefly with the *idea dubia*, but the commentator has a good deal to say about it. He begins by pointing out that a person who is really in doubt can hardly suppose that he knows: an *idea dubia* can hardly be confused with an *idea vera*. And if we press Spinoza's own conception of genuine knowledge, this same criticism may be applied in the case of all three forms of pseudo-cognizance. For they are really very different from genuine knowledge, and it is hard therefore to see how they can be confused with it. "The true idea is an act of knowledge; and, as such, it is an experience true absolutely in its content and certain absolutely in its form. But, if so, the true idea, as the concrete indissoluble unity of absolute truth and absolute certainty, is an experience *sui generis*. It stands alone, unique and unmistakable, sundered utterly from supposals, errors, and doubts; and confusion either way is impossible" (pp. 186-187). But, waiving this radical criticism, we have still to examine the consistency of Spinoza's account as it stands. Joachim finds in this account two conflicting views: (1) that "the mind can only doubt so far as its power of thinking is in abeyance; its state, in doubting, is an unthinking consciousness, an alogical or subrational perplexedness about a perceived or pictured object" (pp. 192-193); (2) that doubt is due to the presence of confusion in our thinking about the object. In face of Spinoza's explicit statements in §§ 78, 80, I find it difficult to understand the attribution of the first view to him, But since Joachim does attribute both views to him he naturally concludes that the theory "falls hopelessly to pieces," for "when

examined in detail, [it] shows itself to be in fact no single, coherent doctrine at all " (p. 197).

Spinoza himself says that in the second part of the method everything turns upon the possession of good definitions; and the greater part of Joachim's unfinished last chapter on 'The Framing and Discovery of Definitions' is devoted to this topic of definition. What Spinoza has to say on the subject assumes the general doctrines of his philosophy, and does not, of course, carry us very far, since he is only giving us a programme, which itself was neither completed nor exemplified in T. His achievement in the way of method must be judged, I suppose, by his performance in the *Ethics*. It seems to me, therefore, that Joachim's criticism of this part of T is unduly severe. He finds here again the contradiction which he had attributed to Spinoza in ch. iii in the earlier discussion of method, *viz.*, that, if the knower must already possess self-evident and self-guaranteed knowledge, then any reflective control by method becomes superfluous. But if Spinoza saw in geometry an example of good method he might fairly argue that, so far as a similar method was applicable in philosophy, it would be attended with similarly good results. There is more justice, no doubt, in the further criticism of Joachim, that, since Spinoza's programme assumes certain philosophical doctrines, his argument is " plausible and persuasive only to those who already know ", and are convinced of the truth of, his metaphysical theory (p. 206). The part of T in which Spinoza speaks about the right ordering of our ideas and the 'series of fixed and eternal things' is difficult and obscure. Joachim, after stating the substance of the passage and commenting upon it, is of opinion that " it is difficult to see any value whatever in this part of the doctrine of method " (p. 223). The chapter concludes with an indication of particular points in the passage which Joachim had meant to discuss more fully.

As I indicated at the beginning, the book is to be taken less as a commentary in the ordinary sense than as a critical examination of the leading doctrines of T construed in the light of the metaphysics of E. So taken it supplements the study of the *Ethics* which Joachim gave in his previous book, and completes his exposition and criticism of Spinoza's philosophy. It is unfortunate that he was unable to give the present book that thorough revision which he had in view; " he had in mind ", we are told, " far more than just a final polish. He occasionally spoke of rewriting." But to the student this matters little, for he can see clearly enough the problems of interpretation and criticism on which Joachim's mind had been working, and he has before him the materials on which any further revision must have been based.

H. BARKER.

Kleines Lehrbuch des Positivismus: Einführung in die empiristische Wissenschaftsauffassung. Von RICHARD VON MISES. Library of Unified Science, Book Series, Vol. I. V. P. Van Stockum and Zoon, The Hague, Holland. University of Chicago Press, Chicago. 1939. Pp. xii, 467.

THE mystic number seven runs through this text-book of positivism, but it is in police uniform. The book has seven parts, each part with four sections, and each section has seven sub-sections. Each sub-section is followed by a brief summary of the main point made in it, and in sub-section seven of section twenty-eight there is a final résumé of the seven main points contained in the seven parts. (There are 7×66 pages of text and notes, but I think this was undesigned.) The exposition is throughout careful and lucid, and notes and references are collected at the end, so that all the pages are beautifully tidy. All this is characteristic of the author, as is the motto for the book taken from Nietzsche: " . . . die kleinen unscheinbaren, *vorsichtigen* Wahrheiten, welche mit strenger Methode gefunden wurden, höher zu schätzen als jene weiten schwebenden umschleiernden Allgemeinheiten, nach denen das Bedürfnis religiöser oder künstlerischer Zeitalter greift ".

Paper and print are admirable, margins wide, and the whole book is a pleasure to handle.

The author calls it a small text-book; and the ground covered is so wide, the number of questions discussed so large, that only brief consideration is possible of many matters that would deserve longer treatment. But there is nothing perfunctory, and the brief discussions are full of interest, and should serve as the starting-point for the reader's own exploration.

The first four parts, dealing respectively with Language, Analysis, Exact Theory, and Causality and Probability, give the ground pattern of the author's view, and present an excellent picture of modern physicalism; part five considers history, psychology, biology, and the social sciences; part six deals with metaphysics and art, and part seven with ethics, law, and religion. The grouping is significant.

The first four parts cover ground well marked out by modern discussions, and the positions adopted have been largely influenced by Wittgenstein and the Vienna Circle. Von Mises regards his position as nearer that of Philipp Frank; he differs on many points from Carnap and Neurath, being much less radical.

His main preoccupation, like that of the physicalists, is with language. He is concerned with an examination and analysis of human statements rather than with an analysis of the human understanding. This examination is conditioned by his idea of the aim of science, which is to get a method which will enable statements to be made which are clear and unambiguous, able to be accepted by all persons who are in a position to test them, and of practical

value in enabling men "to discover connexions between observable appearances, so that from a partially given complex the remaining elements can be built up in thought" (pp. 426-427.) He accepts the views of the logical positivists that the most elementary statements which can be made are the so-called protocol statements, "short linguistic references to immediate present fact of observation"; that all other statements stand or fall according as they are or are not in principle reducible to protocol statements; and that this reduction may involve long chains of reasoning, which depend on the construction in special fields of special systems of axioms and deductions therefrom. Axioms state what special signs may be used in such a system, what combinations of these signs make permissible sentences, and what types of new sentence can be made, consequent on certain others. Definitions merely explain the use of certain abbreviations. Formal deduction from the axioms consists in the manipulation of these sentences in accordance with a special set of rules—those of logic—for combining sentences into tautologies. Systems of axioms and the theorems deduced therefrom say nothing about the world, though if they are to be of practical use, the axioms themselves must be not unconnected with observation. Hypothetical generalisations derived from observation of fact form the rough material which suggests the axioms, though the form which the axioms take is not uniquely determined by the rough material. Thus arbitrary elements enter into all theory. The correspondence between the axioms and observations is never exact; but the aim is to build up a system of axioms of such a sort that from the sentences deduced in the system, along with certain statements about actual occurrences, hypothetical statements about other actual occurrences can be derived, capable of being tested by observation. This general account holds, Von Mises thinks, not only of physics, but also of logic and mathematics.

If this general account is correct, then mathematics cannot be appealed to as evidence that there are *a priori* synthetic propositions, or as evidence that men can intuit general truths about the universe. All statements about actuality express either observations of fact or hypothetical generalisations. Thus one of the main supports of the traditional claims in metaphysics and ethics is cut away. The links in the chain of scientific investigation are: protocol statements, hypothetical generalisations, axioms (partly arbitrary) and deductions therefrom, new generalisations leading to new protocol statements. With the clearing up of this situation, it is claimed, there disappear those mysteries which made philosophers in the past speculate about "insight", "reason", "synthetic apprehension", "understanding", etc. The scientist's task is description of what occurs, and he has no other task. "There is no synthesis other than the totality of what we have to say."

In carrying out this task, the special sciences develop their own special languages, which incorporate much of the language of

ordinary speech, but add many technical terms, and also disallow some types of statement which ordinary language permits. The kinds of combination of words into sentences permitted by a particular language are a matter of usage. It is not easy to codify them precisely, for sometimes they are not precise; but an account of them would form the grammar of the language; *e.g.*, that a noun or its equivalent is needed to serve as the subject of a verb. Von Mises distinguishes between ordinary grammar and logical grammar, noting as a preliminary example that ordinary grammar does not rule out such a sentence as "The table eats sugar", while logical grammar would insist that only a noun indicating a thing capable of activity could be the subject of this verb. Ordinary language permits many combinations which the specialist, whether in logic or in some branch of science, is forced to disallow in order either to avoid ambiguity or to secure modes of expression in accordance with his theories about the relations between occurrences. With words like "is", "of", "since", "because", "causes", etc., ordinary speech permits a variety of constructions on which the logician turns a critical eye; and the result of his prunings, and of his introduction of various new signs in order to express distinctions where ordinary language makes none, is a modification both of meanings of words, and of permitted usages, which constitutes a language at least partially different from the ordinary language. In a similar way the special sciences develop their own special languages.

There are two points in which in the view of Von Mises the logical positivists go beyond what he is willing to admit: (i) They think it possible to lay down finally the rules of logical grammar for a scientific language; (ii) they think that we are at present in a position to see that a single language will serve for all the special sciences ("Unified science"). If (i) is possible, then a distinction can be made between those sentences which are permitted by the grammatical rules and those which are not, the latter being meaningless. Genuine problems can then be distinguished from those which are insoluble because they have no meaning, and one of man's age-long worries can be finally got rid of. (i) is, I think, bound up with (ii), because it supposes that we are at present in a position to state definitely what are the main types of relationship holding between the contents of observed occurrences. It is in the true spirit of positivism that Von Mises is unwilling to accept either of these two points. We know from the history of human thought what radical changes of outlook are possible as the result of new knowledge of fact; we have no reason to suppose that our own outlook is immune from future changes, and we do not know of what kind these changes might be. Until a final account of fact has been given—"until all has been said"—we are not in a position to say what are the types of relation holding in fact, or to lay down rules prescribing what can and what cannot meaningfully be said.

Hence instead of a distinction between sentences having significance and those having none, Von Mises prefers to work with a concept which is relative to particular selected sets of linguistic usages —that of connectibility (Verbindbarkeit).

His primary account of it is this. A sentence (whether true or false) is " connectible " " when it is in accordance [in Einklang steht] with a determinate set of statements which regulate the use of the words and the groupings of words which enter into it " (p. 66). It is not necessary that this regulation should be effected through a set of explicitly formulated generalisations. *E.g.* the statements made in a specialist science are such that taken together they determine the uses and the groupings of the words entering into them, so that a set of rules could theoretically be formulated. If these rules were formulated these statements in the science would be connectible with them ; and so far as they can themselves be taken as determining the rules, they can be said to be connectible with one another. Thus it appears that the word " connectible " can be used in two ways. We can say that a sentence p is connectible with the set of linguistic rules R_1, R_2, R_3, etc., or we can say that the sentences p, q, r, etc., are connectible with one another (in so far as they are all connectible with the same set of linguistic rules). The sentences of a special science are, says Von Mises, usually connectible with the majority of the common general rules for the ordinary use of speech (though this will not be the case where words of ordinary speech are given more restricted meanings in the science). The statements in one special science are not always actually connectible with the statements made in a different special science, though this is an aim always kept in view. Metaphysical statements, which Von Mises will not with Carnap brand outright as meaningless, are, he says, often connectible with other statements only within a very narrow range. Connectibility is prior to verifiability, since the testing of a statement involves a knowledge of the rules determining the meanings of the words and constructions in the statement. Again many statements, connectible with the rules of ordinary speech, serve the practical purposes of that speech, although it is not possible to give an account of any method by which they could be verified—*e.g.* such a statement in ordinary language as " X has a good character ", or again universal statements, which cannot be verified, although it may be possible for them to be falsified.

The use of this concept of connectibility makes it possible for Von Mises to give a sympathetic account of many types of statement which do not reach—do not attempt even to approach—the kind of verifiability he regards as required in the advanced sciences. In the course of his book, he considers the conditions which give rise to statements in ordinary speech (Part I), in the so-called sciences of mind (Part V), in philosophy and in poetry (and the plastic arts and music, where an extended use of terms permits them to be treated on lines parallel to his treatment of poetry) (Part VI), and finally in

law, ethics, and religion (Part VII). It must suffice to give very brief indications of his views. We can perhaps say that he considers all these departments of human thought in the light of his ideal of scientific knowledge, although he disclaims any attempt at evaluation. Ordinary language, *e.g.*, preserves primitive theories of the world, and its structure standardises relations between features of appearances singled out for the purposes of everyday life; it would thus be a mistake to take it (as some philosophers do, Von Mises thinks) as revealing the nature of things. A critical attitude toward it is necessary, if advance in knowledge is to be possible. But while ordinary statements do not satisfy the requirements of strict science, they do on the whole do the work for which they were intended, that of communication for practical purposes. They are connectible among themselves, although the individual words and particular constructions are unambiguous only within particular ranges of application, being vague and indeterminate beyond these ranges. As for the sciences of mind and the historical sciences, Von Mises is unable to see anything, either in the nature of their subject-matter or in their mode of treatment, which would justify any radical distinction of kind between them and the physical sciences. He discusses this question in some detail in Part V. He expects history as knowledge advances to give a larger place to generalisation. That historians are theorists constructing hypotheses is, he thinks, suggested by the fact that very often a reconsideration of the evidence without the discovery of new sources involves a rewriting of the history.

Metaphysics and poetry he regards as playing, each in its own way, a useful part in preparation for future scientific work. Problems of wide human interest which the sciences are not yet in a position to handle are treated by metaphysicians; and although their methods are inadequate, depending on the assumption that certain words of ordinary speech have an exact objective significance the determination of which will furnish information about the structure of " reality ", yet their work serves to keep these problems in view.

Poetry and literature have a different method, but these, too, he sees as preparing the way for science, and sometimes as serving as a corrective to a science which has become over intent on some particular aspect of occurrences, and tends to be dogmatic about the rest. In an age which is in danger of being dominated by some particular scientific dogma the poet can bring attention back to features of the world of appearance neglected or maltreated by the scientists, and may even point the way to a better scientific treatment of them.

In his discussion of poetry Von Mises starts with a quotation from Rilke to the effect that it is not sufficient to describe poetry as resulting from feelings, but that behind these feelings is a multitude of experiences which were very clearly and sharply realised,

each in its specificity and uniqueness, but are no longer distinctly remembered. " Erst wenn sie Blut werden in uns, Blick und Gebärde, namenlos und nicht mehr zu unterscheiden von uns selbst, erst dann kann es geschehen, dass in einer sehr seltenen Stunde das erste Wort eines Verses aufsteht in ihrer Mitte und aus ihnen ausgeht " (p. 336).

For Von Mises the artist gives expression to aspects of fact not yet adequately dealt with by the scientist, building up a special language in the process. " Eine eigens ausgebildete sprachlogische Methode, die der Dichtkunst, ermöglicht — gelegentlich unter Ausnützung klanglicher Nebenwirkungen — die Darstellung solcher Inhalte, *d.h.* die *Übertragung von Stimmungen und Gefühlen mithilfe sprachlicher Mitteilung* an Leser oder Zuhörer " (p. 342). Von Mises brings the plastic arts under the same general principle. His two main points are: (i) that the artist is giving expression to judgments. " Nicht Täuschung, sondern Theorie " (p. 345). " Jedes Bild, jede künstlerische Schöpfung ist eine *Theorie eines bestimmten Ausschnitts aus der Wirklichkeit* " (p. 347). (ii) That in this expression, in literature and in the plastic arts, special " languages " are in the course of time developed for the purpose; they have to be learned by acquaintance with the works of art and with the appropriate facts of experience. The artist, in this view of him, has no touch with any " higher " truth than that reached by the scientist; his statements are statements about fact, made in a different way from those made by the scientist, but they are just as much an expression of hypothesis as those of science, and they are forerunners of future scientific statements. Statements made in the language of poetry cannot be translated without remainder into the language either of everyday speech or of science (p. 428); this implies, I take it, that they are not entirely connectible with the language of ordinary speech or with the language of science.

While thus putting science in the centre of his picture, Von Mises is true to the general spirit of positivism in refusing to dogmatise about the future. Comte's law of three stages he objects to because it makes too definite a prediction as to the course which will be taken by investigations in the future (pp. 417-418). Other ways than that of positive science may at some epoch turn out to be more fruitful at that epoch. We need not attempt to predict. The sensible course is to go ahead on the lines that seem to us to be most fruitful.

On questions of law and ethics Von Mises follows a line we should expect of a positivist. Just as there is no such absolute quality as beauty, so there is no such absolute quality as good. Particular individuals make decisions about something to be done, and sometimes carry them out; they also endeavour to compel or to persuade other individuals to follow certain lines of action. All this, influenced by the decisions and ideas of the community in which they live, gives rise to systems of rules of conduct, which come to be thought of without explicit reference to any individual or body of

individuals laying them down. " The basis of a system of rules can consist only of statements which express connexions between observable appearances (human activities and their consequences) which therefore belong to the empirical sciences, especially sociology " (p. 428). The decisions themselves are facts of human behaviour. Von Mises is averse to generalising about the conditions under which these decisions are made. Different bodies of men have different views about the most satisfactory kind of social life.

Systems of religion, the author thinks, are attempts at accounts of the world order, akin to those of metaphysics and to certain artistic creations, made with the special object of getting principles regulating man's moral life, with sanctions of reward and punishment hereafter. While he does not think any of these attempts " meaningless ", he expects that in the future human life and the relations between men will be ruled to an increasing extent by scientific knowledge (p. 428).

I have restricted myself to giving a general view of the positions expounded in this book. It is full of excellent discussions on particular questions in all the fields over which it ranges. I know of no better introduction to the general school of thought which it represents. It is to be hoped that it will soon be translated into English.

L. J. RUSSELL.

A Philosophy of Religion. By EDGAR SHEFFIELD BRIGHTMAN, Borden Parker Bowne Professor of Philosophy, Boston University. New York: Prentice Hall, Inc., 1940. Pp. xvii + 539. $4.00.

PROF. BRIGHTMAN'S key-words are " experience " and " value ". In the opening chapter he defines religion as " concern about experiences which are regarded as of supreme value; devotion toward a power or powers believed to originate, increase and conserve these values; and some suitable expression of this concern and devotion, whether through symbolic rites or through other individual and social conduct " (p. 17). Philosophy of religion is defined (p. 22) as " an attempt to discover by rational interpretation of religion and its relations to other types of experience, the truth of religious beliefs and the value of religious attitudes and practices ". It differs from religion in that it concentrates on truth, to the exclusion of practical and emotional interests; and from the sciences (history, psychology, sociology) that take note of religious phenomena, in that it knows no departmental limitations and is concerned not merely with facts but with values. These definitions are avowedly heuristic and find their justification in the course of the ensuing argument. The author disclaims any pretension to finality. There can be no

philosophia perennis. What he offers is *a* philosophy of religion, a hypothetical construction as a stepping-stone to further truth. "Only God, or some one who confused himself with deity, could write *the* philosophy of religion" (p. vii). He is out to state positive convictions and he does so with engaging frankness; he relies throughout on philosophic argument and shows scrupulous fairness in the presentation of views differing from his own. He discriminates with a sound judgement the relative strength and weakness of the grounds alleged for or against a given doctrine, *e.g.* personal immortality (c. xiii). The scheme of arrangement is admirably clear. The first four chapters are introductory, dealing with provisional definitions (c. i), the method appropriate to the inquiry (cc. i and iv), a summary of the results of the sciences descriptive of the facts of religious experience (c. ii), and religion as experience of value (c. iii). The main section of the work (cc. v-x) is devoted to an examination of the belief in God. It comprises a critical review of the various conceptions of God (c. v) and of the ways in which men have claimed that God can be known (c. vi); and a discussion of the two problems of a rational belief in God (c. vii) and of disvalue and evil in the universe (c. viii). "All argument about God remains formal until its concrete application to the goods and evils of life is made clear" (p. 239). There follows an acutely reasoned trial of the issue between the hypothesis of a God who is absolute and omnipotent and that of a "finite-infinite Controller of the Given" (cc. ix, x), in which the latter is adjudged victorious. In chapters xi-xiv, the author considers the religious relationship on the human side, treating of human personality (c. xi), human purposiveness (c. xii), human immortality (c. xiii), and the validity of man's religious experience (c. xiv). The two remaining chapters are of the nature of an appendix, dealing somewhat summarily with internal and external criticisms of religion. There are two good bibliographies, historical and general, and a full index. Here, and throughout the volume, prominence is naturally given to modern American writers, with the result that references, and even quotations, are sometimes drawn from philosophers of the second rank. Prof. Brightman's fondness for tabulating the *pros* and *cons* for various doctrines (though never without a brief indication of their rational grounds) offers a dangerous temptation to the elementary student. We picture a candidate on the eve of an examination pouncing avidly on the array of points bearing on the results of the special sciences (c. ii), on the problem of evil (c. x), or on that of immortality (c. xiii), and, after an hour or two of unwonted concentration, proving himself, in the words of an old Oxonian parody, to be "more than three examiners could plough from morn till night". Prof. Brightman, whose conception of God has much in common with Dr. Whitehead's, might have gathered more leaves from the *selva oscura* of that philosopher, as regards the manner as well as the matter of his exposition.

We shall return to God presently. We have first to note certain

ambiguities in Prof. Brightman's use of the blessèd word ' experience ', which has been made to shoulder so much responsibility in modern philosophical literature. His method is empirical, in that, unlike a-priorism, logical positivism, and Barthian super-naturalism, it takes any and all experience as its *datum*, without restriction or *parti pris*, examining its claims at the bar of reason (c. i, § 2). Realising the need for a definition, he tells us that " experience, as used in this book, is a word that refers to the immediate data of consciousness; whatever is present in consciousness is said to be experienced; what at any given time is not present in consciousness is not experienced " (p. 163). Experience, then, includes what is presented in consciousness as well as the consciousness of what is presented, the ' ed ' as well as the ' ing '. So we read of experience being the ' field of consciousness ' (p. 164), of the ' empirical situation ' (p. 234), and of the " sense-qualities of physical things " as " the ' content ' aspect of the Given in God's experience " (p. 332). In Alexander's terminology, it is not a case of enjoyment as distinct from ' contemplation '. But the ed-factor in question is not the world of objective things and persons. These are hypothetical entities, inferred from direct experience (p. 238 *n.*). " Objects known ", we are told, are " always numerically distinct from the idea that knows them " (p. 164) (a queer expression this last !). The content of experience is mental (p. 413); save in the case of the self, it consists of essences and carries no guarantee of existence. With this single exception, Prof. Brightman's position is that of Santayana and the Critical Realists (p. 165). " Our belief in other minds, and so in society, is a reasonable hypothesis for the explanation of our experience, but it is not one that can itself be experimentally verified. . . . Experimental verification, without use of reason, cannot give us society or nature or God; it leaves us shut up in solipsism " (p. 322). Even if we accept the truth of this epistemological dualism, despite its obvious difficulties, why must what is thus presented in consciousness, *e.g.* sense-qualities, be mental ? Is there any shadow of a ground for the assertion that " the only situation experienced by anyone " is confined to " his own consciousness " (p. 347) ? In more than one passage Prof. Brightman seems to forget the essences and values which form the content of experience and to identify consciousness with mental acts; as in his definition of ' the situation experienced ' as " a self-experiencing whole which includes thinking, choosing, remembering, anticipating and purposing, as well as feeling and sensing " (p. 348), or in the statement (pp. 363) that consciousness is imperceptible by sense, since " no one ever had a sense-perception of thinking, or feeling, or choosing, or even of sensing itself ". [Yet in the preceding sentence we are told that it is a ' debateable ' question " whether we perceive only our own sensuous consciousness "!] All this is terribly confusing; and the confusion is worse confounded in Prof. Brightman's account of knowledge. Experience gives certainty but not knowledge. Its " reference to a supposed object "

constitutes indeed a "knowledge-claim". But it "becomes knowledge only when (*a*) it is rationally organised, and (*b*) it refers beyond the moment's experience to something else" (p. 315 *n.*). He speaks of experience as containing a double reference, to objects other than itself and also "to our own consciousness as we have it" —a use of the term 'reference' to which the present reviewer can attach no meaning. The outcome is that when we have certainty we have no knowledge, not even of the existence of the self, to say nothing of apprehended essences and values; and when we have knowledge we have no certainty. Is not this enough to make Descartes turn in his grave?

It all comes of the vain search for an irrefragable *datum* of experience to serve as the groundwork for reasoned knowledge. *Data* may exist for the special sciences, though even these are critical of the 'given' facts; but philosophy knows nothing of such entities. As Prof. Hallett has put it in a recent article in MIND: "we must begin where we find ourselves: not with passively accepted *data* . . . but with *problemata* to be faced and engaged in a never-ending speculative and exploratory activity".[1]

It follows that for Prof. Brightman God is known, like other selves, by inference and not by experience. He is known as object (p. 342), not as subject (though in prayer He is addressed in the second person) a view that rules out of court one of the main contentions of contemporary Protestant theology. The conception of God unfolded in the central chapters is the most interesting and carefully reasoned construction in the volume. Experience of value is the foundation for a rational belief in God; experience of disvalue and evil for the belief that He is temporal and finite. In the former of these positions, the author is on familiar ground: and we content ourselves with noting the ambiguity in the statements that God is the supreme value (p. 179; *cf.* p. 319, "the goodness in the universe"), the inclusive "system" of all values, and the Creator, source, and goal (p. 179) of value. He can hardly be all these at once. The Being who creates and sustains and promotes value cannot Himself be identified with value. At least, not by Prof. Brightman, who holds that values, like other essences, are indifferent to actual existence. His intention clearly is that God, an actual existent, is the author and bearer of value, through whom the dualism of fact and value, existence and essence, is finally overcome; a view closely akin to Dr. Whitehead's. On the other hand, the facts of evil preclude equation of God with the Absolute. The frank recognition of these facts, and of their bearing on God's perfect goodness, is perhaps the most impressive feature of Prof. Brightman's book. That God is wholly immanent in the universe and shares in its temporality is taken for granted without sufficient argument; and we look in vain throughout these pages for a reasoned critique of the classical doctrine of eternity as meaning transcendence of time. Yet it is

[1] MIND, *July*, 1940, p. 306.

difficult to conceive of the Creator and bearer of timeless essences as Himself other than timeless. The modern mind, and especially that of America, shows a marked reluctance to allow timeless being to any existent entity. Dr. Whitehead, it is true, succeeds, thanks to his dark distinction between the primordial and the consequential natures of God, in both running with the hare and hunting with the hounds; but few have found themselves able to follow, or even to understand, his doctrine. What Prof. Brightman stresses most is that God is finite, or rather "finite-infinite"; "strictly we should speak of a God whose will is finite rather than a finite God; for even the finite God is absolute in the sense of being the ultimate source of all creation" (p. 337). Moreover, He is infinite in respect of his duration throughout time. "God is personal consciousness of eternal" (= everlasting) "duration; his consciousness is an eternally" (= everlastingly) "active will, which eternally finds and controls the Given within every moment of his eternal experience" (pp. 336, 337). What, then, is the Given that conditions God's activity? Prof. Brightman's answer to this crucial question seems to raise difficulties graver even than those which it claims to solve. The Given is "an aspect of God's consciousness which eternally enters into every moment of the divine experience and into everything that is, either as obstacle or as instrument to the will of God" (p. 300). It "consists of the eternal uncreated laws of reason" (the *impossibilia in se* of Aquinas) "and also of equally eternal and uncreated processes of non-rational consciousness which exhibit all the ultimate qualities of sense objects (*qualia*), disorderly impulses and desires, such experiences as pain and suffering, the forms of space and time, and whatever in God is the source of such evil" (p. 337). "The reality of the physical universe is located wholly within the conscious experience of God. Physical space-time is God's standard space-time experience; energy and force are God's will controlling and directing this experience; the sense-qualities of physical things are the "content" aspect of the Given in God's experience, while the mathematical formulæ of physics result from God's will that his given "content" shall be controlled by "form". Biological evolution is God's progressive creative control of the Given, partly successful, partly unsuccessful" (p. 332). The cosmic process is immanent in God's being; it is expressly denied that his creativeness is productive of what is external to himself (*ib., n.*). Prof. Brightman does not appear to recognise any distinction between externality and otherness. His 'Given' is reminiscent of Aristotle's indeterminate ὕλη, transferred in wholly non-Aristotelian fashion as an integral factor into God's nature. Have we here a reasonable answer to the problem of evil? The Given, be it noted, is in large measure a source of value as well as of disvalue, the laws of reason and sense-qualities being referred to it. As in the case of Aristotle's ὕλη, we are at a loss to discover what it is in the Given that can account for the positive mass of evil and suffering in the universe.

It functions as an *asylum ignorantiae* and nothing more. It is true that God escapes moral responsibility for the existence of disvalue, but at the tremendous cost of admitting irrationality at the very heart of his being. Prof. Brightman's claim to avoid dualism cannot stand against this ultimate diremption of the divine nature. He eludes the obvious speculative objections to Manichæism only by reintroducing dualism within God. And if the source of evil lies in God's irrational and non-voluntary nature, how can it be maintained that he is "infinitely good"? (p. 401). If he is limited anywhere, he is limited all along the line. And, if God's will is wholly good, what meaning can we attach to the statement (p. 401) that he is under "obligation" to maintain in existence the human persons whom he has created? Otherwise, we are told, "God would be in an unenviable position. He would either continue forever to create new persons, or he would give up the enterprise of creation" (*ib.*). Yet he is in an unenviable position in any case, more so indeed than we are ourselves; we are up against evils imposed upon us from without, but God nurtures them, however unwillingly, within his own bosom. But the gravest objection to Prof. Brightman's solution of the problem is that it conflicts with the evidence of religious experience, wherever religion has risen to a high level of reflective thought. That God is transcendent of the works of his creation and omnipotent, is common ground to Judaism, Islam and Christianity. Prof. Brightman hardly does justice to the faith in God's transcendence, interpreting it mainly in the light of Karl Barth's antirationalism to the neglect of the main historical current of Christian theology. Like so many Protestant writers, he shows an imperfect acquaintance alike with Catholic doctrine and Catholic devotion. Else how could he write that "the living religion of the Catholic Christian does not centre on the theological concept of Saint Thomas's God; but it feeds on finite objects—the crucified Jesus of Nazareth, the Virgin Mary, and the saints" (p. 325). Is there any serious evidence to show that a finite God is a possible object of worship for the developed religious consciousness? The author's criticism (p. 325 *n.*) that the judgements of McTaggart (who rejected Theism) and of Hocking (who accepted it) "regarding the religious worthlessness of finitism is so contrary to experience and so casually grounded as to arouse some surprise" could be more fitly turned against his own position. He contends that a God who transcends time must be "utterly unrelated to what is revealed in experience" (pp. 385, 386). This objection doubtless holds, if we rule out of court a revelation *ex parte Dei* of what admittedly exceeds the grasp of human understanding. But Prof. Brightman is himself prepared to ascribe to his immanent and finite God the power of responding to human prayer and worship. Why should this capacity be denied to a God who is infinite and transcendent? The author never clearly faces—and surely it should be faced in a work on the philosophy of religion—the Christian claim that its divinely

revealed doctrines of creation and redemption offer to men's imperfect comprehension a more *reasonable* solution to the problem of evil than any other alternative, and one that at the same time is in harmony with living religious experience.

We have dwelt in this review on the objections to Prof. Brightman's argument rather than on its merits. Its merits—breadth of outlook, fairness to opponents, honesty of conviction, intellectual integrity—are the author's own; the defects are those of his people and his time. It is easy for him to answer the criticism (pp. 328, 329) that his finite-infinite God is simply "a glorified college professor". A far better analogy would have been "a glorified President of the United States". We have no desire to minimise the difficulties that beset any form of theistic belief. But if theism be true, its truth will hardly be vindicated by conceiving God as wholly immersed in the flux of temporality, or man as qualified, by his natural powers of reason and will, to assist his Maker in a precarious struggle to conserve the value and triumph over the disvalue in the universe.

W. G. DE BURGH.

VI.—NEW BOOKS.

The Problem of the Future Life. By C. J. SHEBBEARE, D.D. Oxford: Basil Blackwell, 1939. Pp. xii + 96. 2s. 6d.

THIS book, to which the Dean of St. Paul's contributes a preface, is intended for the general reader rather than for the professional philosopher or theologian. It deals with various aspects of the Christian hope of immortality, and attempts to present a picture of the Future Life which would be both in accordance with the Christian beliefs and also intellectually and emotionally acceptable to the modern reader.

To anyone who has thought on the subject, its enormous difficulties are familiar; and it is a great merit of Dr. Shebbeare's book that it tries to deal honestly with the matter and does not shrink from discussing issues which are usually evaded. Some problems seriously discussed in the book (like "would two friends who had buried their enmity in a woman's grave, still be friends at the resurrection?" and whether we shall all be naked in Heaven) will, no doubt, seem ludicrous to those who are used to thinking on entirely different lines. But this must not, I think, obscure the value of Dr. Shebbeare's discussion: he is trying to face frankly all the issues which may occur to the ordinary Christian believer.

The first few chapters are introductory; the two most important of these deal with the conception of Eternal Life and that of Absorption into God. I am afraid that I find these chapters, as also some later ones on Eternity and Idealism, extremely obscure. For instance, Dr. Shebbeare suggests that God may stand to me as my present stands to my immediate past: that the last member of this succession pours its memories into Him as my earlier memories are received by my present self: so that God is, so to speak, the very next stage of my personal history. And similarly, of course, He is the next stage of Smith's personal history and of Brown's and of Robinson's. At the same time, Dr. Shebbeare clearly believes in *personal* immortality; that is, he believes that each of us will, after the death of our present bodies, continue to exist as a *separate* personality. As I do not really understand Dr. Shebbeare's conception of Absorption into God, I am not able to say whether or not it is inconsistent with this belief. But so far as I can make out it would seem to be so.

Dr. Shebbeare then proceeds to discuss the nature of *evidence* advanced for the belief in the Future Life. He first mentions the evidence of Psychical Research: he does not ignore it, but does not think that it will carry us very far. I dare say it won't; but I do think it needs a much fuller discussion, even within the limits of Dr. Shebbeare's short work, than is afforded to it. Next he mentions the evidence of the Bible, and some old metaphysical arguments, and also Kant's ethical argument. But it is on theism, says Dr. Shebbeare, that the belief in the Future Life rests. If we believe in a God who is both good and omnipotent, we cannot

fail to believe that He will not disappoint our rightly cherished hopes, *e.g.* our hope of further moral development. If there is no Future Life then, for one thing, evil will be conquered only very imperfectly; and the only rational justification of evil could be that it exists to be conquered, *i.e.* that it is a condition of something else which is intrinsically good. In general, if there is no Future Life then the world cannot be regarded as morally good, and must seem incomplete and unsatisfactory to reason.

This account, though inadequate as a summary, gives, I hope, a fair idea of Dr. Shebbeare's arguments. I cannot discuss them here as fully as I should like, and I shall limit myself to the following comments. First, I cannot see that, unless human beings survive, the world is necessarily evil or must necessarily be regarded as incomplete and unsatisfactory. In other words, I cannot see that universal mortality is inconsistent with the belief that there is a moral and omnipotent God, and therefore I do not think that immortality can be deduced from this belief. Secondly, I do not think that Dr. Shebbeare has given any good reasons for maintaining that the *amount* and *kind* of evil which—whether we are immortal or not—certainly does exist is compatible with the existence of a moral and omnipotent God. I do not say that it is *not* compatible, but Dr. Shebbeare has certainly failed to convince me that it is.

In the remaining chapters of his book Dr. Shebbeare considers, amongst other things, the problem of punishment and the intermediate state, and also discusses whether all our emotions and reasonable desires will be satisfied in the Future Life. This discussion does not strike me favourably; partly because the views to which Dr. Shebbeare inclines are obviously connected with our wishes, and also because there is, of course, no good evidence whatever which would enable us to say anything specific about what the Future Life is like, if indeed there is a Future Life at all.

CASIMIR LEWY.

A History of Indian Philosophy. By S. DASGUPTA, Vol. III. Cambridge: At the University Press, 1940. Pp. xiii + 614. 35s.

THE third volume of Dr. Dasgupta's history continues the thread of idealistic thought that closed the first volume. That was the system of Sankara, the system that finds reality only in an undifferentiated one, all the rest being illusion. It is an injustice to Indian thought that Western writers have usually taken Sankara to represent the most characteristic thought of India, and have set it up as a beautifully clear system, but of no actual value in relation to the thought of the West. Nor, on the other hand, did Deussen do Indian philosophy any good by representing Sankara's system both as the one true interpretation of the Upanishads and also as the Indian expression of the ultimate and final truth to be found in Schopenhauer. It is true that by denying the reality of sense-experience this system escaped the necessity of trying to explain the problem of the one and the many, but against it are ranged all the other systems that treat experience as in some sense real. This volume should do much to show that many of the real problems of experience have been apprehended and cogently discussed by Indian thinkers.

The chief system here expounded is that of Ramanuja and related schools; and half the volume is devoted to them. They claim to be monistic, but their monism is qualified. For Ramanuja reality is not an

undifferentiated one. Experience is not mere unreality, and hence many problems have to be discussed that are evaded in Sankara's position—the conception of cause and effect as real, the establishing of a causal theory against the views of other schools, the theory of error, the validity of knowledge, and the nature of soul. This last problem is not merely psychological. The individual is real and remains such, but his relation to God and the whole has to be explained. Hence Ramanuja's system has to include theology. There are in India no separate theologies opposed to philosophies, compact logical systems, and the author has not treated Ramanuja's philosophy as a system to be explained merely from its own premises. Ramanuja was a South Indian, and needs to be taken in connexion with the forms of religion that preceded him in South India. Hence the author has given us a valuable and entirely original description of several of the forms of South Indian religion that contributed to the direction and development of Ramanuja's thought.

Another important and quite original part of the work is the treatment of the philosophical ideas to be found in the Puranas. These popular works include cosmologies, and from them it is possible to see how the current philosophical notions were reflected in them. The question whether we have here traces of earlier forms out of which the formal systems arose, or mere reflections and perversions of these systems, has been discussed by German writers, especially in connexion with the similar matter in the Epics. Of this the author does not seem to be aware, so that an attempt at harmonising or refuting these views still awaits an investigator with wider views. In the meantime the author has contributed much material necessary to the solution, and he promises more of it in a further volume. Besides the schools so far discussed there are still others calling themselves Vedantic that are definitely dualistic. It is clear that the two further volumes promised will be needed to do justice to the author's plan.

E. J. THOMAS.

Living Religions and a World Faith. By WILLIAM ERNEST HOCKING. (Hibbert Lectures.) London: G. Allen & Unwin, Ltd., 1940. Pp. 293. 10s.

THE opening words of Prof. Hocking's Hibbert Lecture give us its purpose. "What we have in the world," he says, "is not religion, but religions. Is this a normal situation, or an historical accident, which now, in the interest of world order, requires a remedy?" Seeking to answer this question, Prof. Hocking begins with the consideration of the nature of religion which shows itself amongst the many varieties of religions. Religion, he thinks, must be both universal and particular. Most religions are content with being the religion of a particular race. Buddhism, Christianity and Islam have sought to be universal religions. Some assert that a synthesis of these could form a world religion. But synthesis is often misleading. Terms as fundamental as God, freedom, immortality have very different connotations in different faiths. Shang-Ti, Allah, Jehovah, and the God of Christ's teaching are four differing conceptions all covered by the name God. A mere synthesis of religions is likely to have the same fate as Esperanto, to which nobody objects, yet not one per cent. trouble to learn.

Other ways of reaching a common religion are by one religion displacing the rest. That is unlikely, and would involve the loss of considerable

values belonging to the displaced faiths. Prof. Hocking prefers the way of reconception. If any religion grasps its own essence, it thereby grasps something of the essence of all religions. It finds identities of meaning under different expressions. In so far as we get beneath the local and temporal in the expression of our own faith, do we also reach the underlying truth of all faiths. In other words we find the universal through the particular. Prof. Hocking has a lively appreciation of the values of the non-Christian religions. A good part of the book is taken up in sketching them. Yet for himself, Prof. Hocking is well content with the faith in which he was brought up. He has the sense, rightly enough, that universal religion is no more sufficient in itself without particular religion than is world citizenship without loyalty to one's own country. Prof. Hocking has made a contribution of value to the discussion of a subject upon which there has been much confused thinking. As a general principle, most of us have a sentiment in favour of unity, and the man who believes in God in much the same manner as he believes in the British Constitution, fails to see why all religions should not get together and be one. The devotee of religion will not consent to surrender one jot or tittle of his creed. Both positions are wrong—and right. If we realise we are one in our search for God, we may continue to seek along different paths, as do the research students of science, and like them learn one from another and each from all.

E. S. WATERHOUSE.

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Prospectus of the London School of Economics and Political Science for the Forty-Sixth Session, 1940-41, pp. 146, 1s.

VII.—PHILOSOPHICAL PERIODICALS.

PHILOSOPHY OF SCIENCE, vii. 2 (April, 1940). W. M. MALISOFF. *What is Insight?* [" Insight " includes all methods of dealing with wholes. It is claimed to be a product of empathy and contemplation.] C. H. MILLER. *Vocation versus Profession in Philosophy.* [" . . . the issue is simply whether or not philosophy is by its nature esoteric. The genteel tradition has said that it is; the contemporary, democratic tradition has reopened the question, insisting that it is not." Suggests that philosophers might co-operate in the elaboration of " as many coherent word-systems as possible within a given domain."] G. BERGMANN. *On Physicalistic Models of Non-Physical Terms.* [Illustrates in detail how features of physical " Gestalts " can be interpreted in terms of relations between associated mathematical variables. (A difficult, over-compressed, paper).] P. B. SHARPE. *A Critical Analysis of the Canons of Science.* [" There is only one fundamental and necessary canon of science, the Method of Concomitant Variations. It should be worded more simply because simple statements, if they are accurate and adequate, have more operational value to students than do complex, involved and obscure statements."] W. H. HILL. *Peirce's " Pragmatic " Method.* [Argues, with illustrative quotation from Peirce's papers, that the " pragmatic maxim " led to a species of objective idealism. Peirce's " method of science " . . . " assumes the existence of realities which are independent of what anybody may think about them; they are the objects represented in true opinions, the final opinions to which our thinking must inevitably lead. The reason why our thinking must lead to these final opinions, so that we are making progress towards a knowledge of reality, is that man has an instinctive intellectual insight into the secrets of the universe. . . . What is this method but the very ' *a priori* method ' which Peirce had meant to discard ? " (A useful paper.)] F. B. WILL. *Verifiability and the External World.* [While approving of the verifiability principle in outline, Will is reluctant to admit the senselessness of questions concerning the existence of the external world. Examples are given of scientific assertions which do not admit of direct verification. " The mere presence of difficulties such as these is not a sufficient reason at the present time for refuting the verifiability theory of meaning. But it does point to two conclusions : (1) that this theory should not be treated as a simple philosophical dogma ; and (2) that it is in considerable need of development and clarification in relation to the actual assertions of everyday life, science, and philosophy."] R. B. WINN. *The Nature of Causation.* [The theory of Causation has been traditionally based on two incompatible assumptions : (1) that there can be no action at a distance, either of space or time, (2) " that every cause precedes its effect and may be located at a distance from it." The attempts of other philosophers to reconcile these propositions are discussed and rejected. The contradiction is held to arise from confusing two senses

of ' cause ', (1) an event considered as affecting *another* cause, (2) a *single* event in process of change. " . . . causal connection depends on two triadic factors, one of which requires no time (an event, or cause-change-effect) and another consisting of transmission of energy (effect-motion-cause) during which an effort of one event becomes the cause (or one of the causes) of another event."] G. BERGMANN. *On Some Methodological Problems of Psychology.* [Survey of such topics as " logical behaviorism," the nature of historical laws in psychology, and the structure of pyscho-analytical theory. Written from the standpoint of Logical Positivism.] L. WOOD. *Inspection and Introspection.* [Inspection is " a direct and immediate apprehension of qualia and the relations between qualia " ; introspection is " the apprehension by a knowing subject of items in the same personal continuum." Inspective knowledge is an essential part of psychology since " all other forms of knowledge are ultimately reducible to inspective terms."] M. LAZEROWITZ. *Self-contradictory Propositions* [Denies that sentences of the form $p \,.\sim p$ (*i.e.* the conjunction of a contingent declarative sentence and its contradictory) express any propositions. ' q is impossible ' entails ' q is logically inconceivable.' Thus to understand an ' impossible proposition ' would be to conceive the inconceivable.] L. BYRNE. *An Educational Application of Resources of the Unity of Science Movement.* [Urges that the principles of the Unity of Science movement should be used as a basis for a reformed curriculum in secondary schools. In this way it is hoped to foster the existence of an " educated class " on which " we must depend in large part for the maintenance and further advancement of civilization."]—Reviews.

REVUE NÉOSCOLASTIQUE DE PHILOSOPHIE. Tome 43. (Deuxième série No. 65.) Fév. 1940. (With separate bibliographical supplement.) **A. Forest.** *L'expérience de la valeur.* [What is the nature of our experience of " value " ? A simple answer is that the object of any tendency has " value " when it guarantees the satisfaction of the tendency. Difficulties only arise when we consider " ideal values " and the movement of will as a " reasonable appetition ". We are then tempted to develop a theory of " values " independent of all existence, and to maintain the " primacy " of the idea of value over that of being. So Kant in his treatment of moral value, and more than one recent French metaphysicist of values. This is, incidentally, an inversion of the tradition going back to Plato, which makes the value of an object for us derivative from a *convenientia* of the being of the object with our own being. The essay, which I do not find always easy to follow, is a defence of the Platonic tradition (which is, of course, also that of the great Schoolmen). The central thought is that if one thing has a value for another, this is because the being of each of them is not simply self-enclosed and particular. Both are enveloped in a universal ' being ' in virtue of which that of either may be said to be implied in that of the other. " Value " is thus not extrinsic to and independent of existence, but it is transcendent of the special determinations of particular existences. Hence our most typical experience of it is in the *unio affectiva* of friendship which discloses to us something more than the particular characters of the beloved object, *viz.*, their *co-aptatio* to our own " natures ".] **L. de Raeymaeker** *Recueillement métaphysique.* [When I apprehend my own being in self-consciousness, I am aware of myself as a unitary personality, self-identical under all the diffusion of my various acts in space and time, and as a free, autonomous,

responsible agent. At the same time I am equally aware that though I am thus a 'subsisting being', I am not the whole of "being"; everywhere relation to being other than myself enters into and conditions my awareness of myself as an autonomous person. Thus the same meditations which disclose to me my own being as something absolute, disclose also the existence of a whole order or hierarchy of beings not myself. The autonomy of the self carries with it the consequence that the self cannot be completely explained by reference to the not-self, and similarly the not-self cannot be explained simply by reference to the self. (I take it that this means that neither is reducible to a "function" of the other.) It follows that neither is completely explicable except by reference to a transcendent and infinite cause, God, who is beyond the whole series of finite causes. We conceive wrongly of creation if we think of God as either a "first" cause in the series of efficients or a "last" cause in the series of final causes. The relation of the finite to God is one of *total* and *integral* dependence; the argument for the "contingency of the world" thus understood is the only way in which God can be reached by a finite intelligence. (It is wrong then to think of God as somehow the "complement" of the world or the world as a complement of God. It would be interesting to read a discussion on this reasoning between the author and, say, Prof. Laird.)] **L. Noël.** *Le réalisme critique et le "bon désaccord"*. [A vigorous polemical rejoinder to the recent volume in which M. Gilson charged the philosophy of Cardinal Mercier with "illationism" and consequent unfaithfulness to the realism of genuine Thomism.] **Alphonse de Waelhens.** *Un Symposium de philosophie française*. [Deals with the volume of essays published last year by the École des Hautes Études of the University of Ghent.] **Géraud de Montpellier.** *Psychologie*. **Werner Goossens.** *Philosophie de la religion*. Miscellaneous book notices. *In Memoriam:* Mgr. Ladeuze, Chimique Générale.

VIII.—NOTES.

A NOTE ON THE TEXT OF ARISTOTLE'S *METAPHYSICS*, 1026a14.

THE unanimous reading of the MSS. in this passage (and it is supported by Alexander) is as follows:—

ἡ μὲν γὰρ φυσικὴ περὶ (14) *ἀχώριστα μὲν ἀλλ' οὐκ ἀκίνητα, τῆς δὲ μαθηματικῆς ἔνια* (15) *περὶ ἀκίνητα μὲν οὐ χωριστὰ δὲ ἴσως ἀλλ' ὡς ἐν ὕλῃ· ἡ* (16) *δὲ πρώτη καὶ περὶ χωριστὰ καὶ ἀκίνητα.* (*Metaph.* E 1, 1026a13-16.)

Schwegler (*Metaphysik des Aristoteles*, note *ad loc.*), perversely *contrasting* the parallel passage in *de An.*, 403b15 f., proposed to read *χωριστὰ* for *ἀχώριστα* in l. 14. Ross adopted the suggestion in his edition of the *Metaphysics* (vol. i, p. 355), and since then he has brought his translation (1928) into line with this reading and has referred to it in his edition of the *Physics* (p. 35). The reading *χωριστά*, accordingly, must now be regarded as the official reading, at any rate in this country. But the change is not an improvement, and the reading of the MSS. should be retained.

The objection to the MS. reading is stated by Ross as follows (*Aristotle's Metaphysics, loc. cit.*): '*ἀχώριστα μὲν ἀλλ' οὐκ ἀκίνητα* would be a false antithesis, for the things that are not separate from matter are necessarily things that have movement'. It must be admitted that this *is* a false antithesis. It is inappropriate to say of the objects of Physics that they are inseparable from matter *but* not immovable, because what is inseparable from matter is *necessarily* not immovable. And it is of course impossible to say that they are *separable* from matter but not immovable, because what is separable from matter is necessarily immovable. Consequently, Ross holds that Aristotle wrote *χωριστὰ* here, meaning 'capable of separate existence' in some sense which he does not further explain, but which is evidently different from 'separable from matter'. (I presume he is thinking of the sense in which substances are, while quantities, qualities, relations and the rest are not, capable of independent existence.)

Now there would be no insuperable objection to supposing Aristotle to use *χωριστὴν* in the sense of 'separable from matter' in 1025b28, and *χωριστὰ* in some other sense in 1026a14; though the introduction of the distinction between things which, like *τὸ σιμόν*, are definable only with reference to a particular sort of matter and things which, like *τὸ κοῖλον*, are definable without any such reference would in that case lose most of its point. But it seems to me incredible that he should use *χωριστὰ* in l. 14 in a sense which has no reference to matter and go on in the very next line to use *οὐ χωριστὰ* in the sense which *has* reference to matter: the objects of some mathematical sciences are *ἀκίνητα μέν, οὐ χωριστὰ δ' ἴσως, ἀλλ' ὡς ἐν ὕλῃ*. The antithesis between *ἀχώριστα* and *οὐκ ἀκίνητα* is illogical, certainly; but the antithesis, imputed to Aristotle by the commentators, between *χωριστὰ* in the sense of 'capable of separate existence' and *οὐ χωριστὰ* in the sense of 'not separable from matter' is far worse.

It is better, then, to take the text as it stands, and allow Aristotle to keep to one point throughout the chapter, while admitting that the expression betrays a certain amount of muddle.

This muddle can be explained in the following way. The argument of the whole chapter is intended to lead up to the conception of First Philosophy as the study of *οὐσίαι* which are (*a*) *χωριστὰ* (= separable from matter), and (*b*) *ἀκίνητα*. With regard to these two predicates, there are obviously just four logical possibilities: (1) *χωριστὰ καὶ ἀκίνητα*, (2) *χωριστὰ μὲν ἀλλ' οὐκ ἀκίνητα*, (3) *ἀκίνητα μὲν ἀλλ' οὐ χωριστά*, (4) *οὔτε χωριστὰ οὔτ' ἀκίνητα*. Of the four possibilities, two (cases 2 and 3) would naturally be expressed in terms of . . . *μὲν ἀλλ' οὐ* . . . , and one each in terms of *καὶ* . . . *καὶ* . . . (1) and *οὔτε* . . . *οὔτε* . . . (2).

Now case (2) is impossible for reasons connected with the analysis of change: things that are *χωριστὰ* are necessarily *ἀκίνητα*. Aristotle has theretore three cases to consider: case (1), in which he is chiefly interested, and cases (3) and (4), with which case (1) is contrasted. What could be more tempting than to point the contrast of the inferior cases with case (1), which is naturally expressed in terms of *καὶ* . . . *καὶ* . . . , by stating them *both* in terms of . . . *μὲν ἀλλ' οὐ* . . . ? In order to keep to the facts, however, case (4), which would naturally be stated in terms of *οὔτε χωριστὰ οὔτε ἀκίνητα*, has to be stated in terms of *ἀχώριστα* . . . *ἀκίνητα*. (The faint antithesis between the *assertion* of the one privative predicate and the *denial* of the other, though it cannot of course bear a moment's scrutiny, might serve to disguise the inappropriateness of *ἀχώριστα μὲν ἀλλ' οὐκ ἀκίνητα*.)

Ross asserts that whereas in 1025b28 the reference is to the *form* of physical things, in 1026a14 the reference is to physical things themselves. But (1) can this distinction be presumed where it is not clearly and explicitly stated? After all, the form of a thing is its *τί ἐστι*, so that the relation between them, especially for the philosopher, is very close indeed. Below (l. 32), the business of a science is stated (in reference to *φιλοσοφία πρώτη*) to be to know about its objects *τί ἐστι καὶ τὰ ὑπάρχοντα*. . . . And (2), is there any reason to draw the distinction here? So recently as 1026a5, 6, Aristotle has been pointing out that the *φυσικὸς* is concerned with certain aspects of the soul, *ὅση μὴ ἄνευ τῆς ὕλης ἐστιν*. The soul is the form of an animal body. If the objects of Physics are the *forms* of physical things in 1026a6, may they not still be the *forms* of physical things in 1026a14?

NOTE.—On a previous occasion (in MIND, July, 1933, p. 336, n. 1) I ventured to suggest that the MS. reading was right. But there was then no space for discussion.

D. R. COUSIN.

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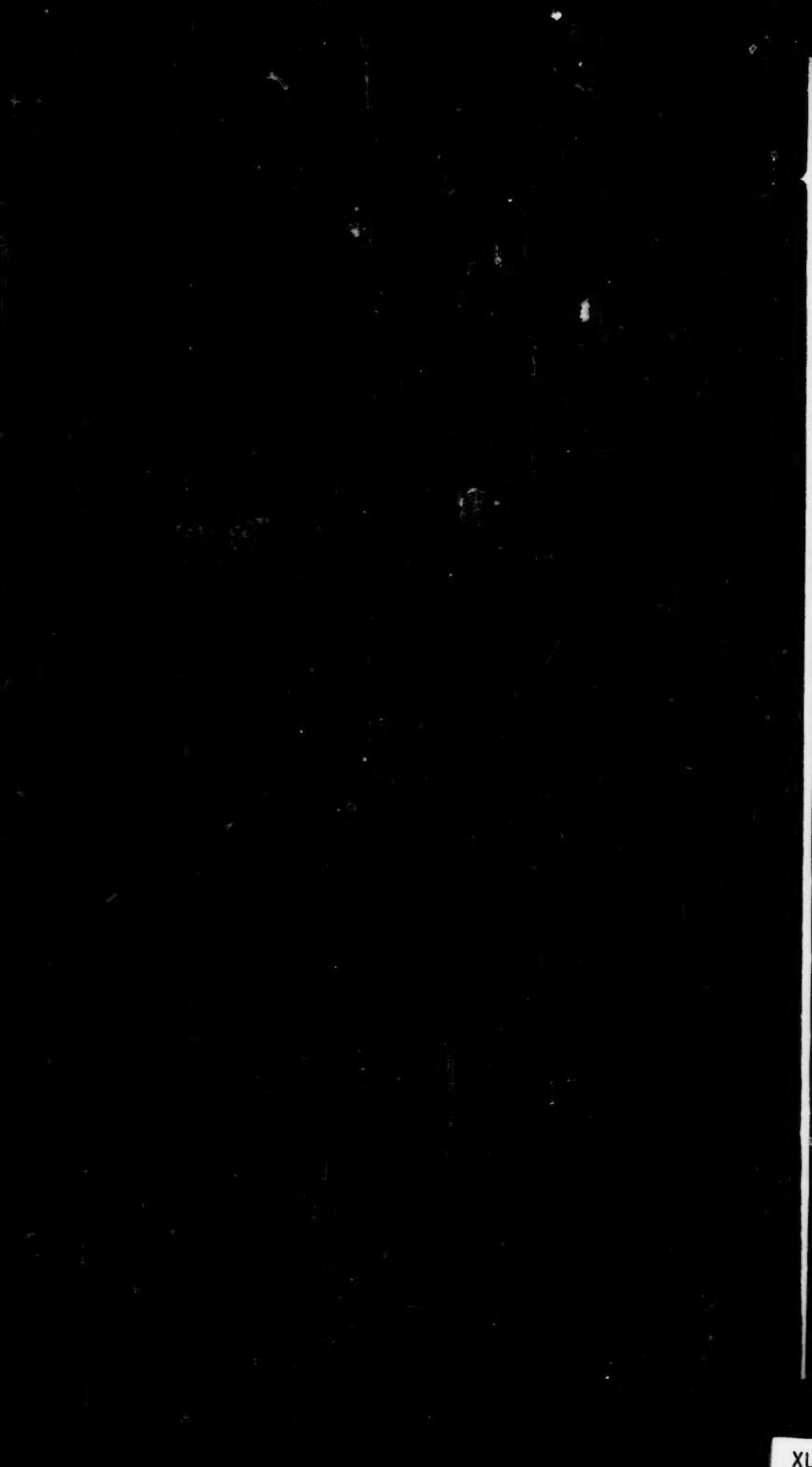

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